BRAIN.

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Original Articles.

PSYCHOMETRIC EXPERIMENTS.

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PSYCHOMETRY, it is hardly necessary to say, means the art of imposing measurement and number upon operations of the mind, as in the practice of determining the reaction-time of different persons. I propose in this memoir to give a new instance of psychometry, and a few of its results. They may not be of any very great novelty or importance, but they are at least definite, and admit of verification; therefore I trust it requires no apology for offering them to the readers of this Journal, who will be prepared to agree in the view, that until the phenomena of any branch of knowledge have been subjected to measurement and number, it cannot assume the status and dignity of a science.

The processes of thought fall into two main categories: in the first of these, ideas present themselves by association either with some object newly perceived by the senses or with previous ideas; in the second process, such of the associated ideas are fixed and vivified by the attention, as happen to be germane to the topic on which the mind is set. In this memoir I do not deal with the second process at all, so I need not speak more in detail concerning it, but I address myself wholly to the first. It is an automatic one; the ideas arise of

their own accord, and we cannot, except in indirect and imperfect ways, compel them to come.

My object is to show how the whole of these associated ideas, though they are for the most part exceedingly fleeting and obscure, and barely cross the threshold of our consciousness, may be seized, dragged into daylight, and recorded. I shall then treat the records of some experiments statistically, and will make out what I can from them.

I should be glad if the reader would refer to an article written by me in the 'Nineteenth Century' of last March, which was based on the observations I am about to describe. It travels somewhat further afield than the present memoir, but does not enter so much into details.

When we attempt to trace the first steps in each operation of our minds, we are usually baulked by the difficulty of keeping watch, without embarrassing the freedom of its action. The difficulty is much more than the common and well-known one of attending to two things at once. It is especially due to the fact that the elementary operations of the mind are exceedingly faint and evanescent, and that it requires the utmost painstaking to watch them properly. It would seem impossible to give the required attention to the processes of thought and yet to think as freely as if the mind had been in no way preoccupied. The peculiarity of the experiments I am about to describe is that I have succeeded in evading this difficulty. My method consists in allowing the mind to play freely for a very brief period, until a couple or so of ideas have passed through it, and then, while the traces or echoes of those ideas are still lingering in the brain, to turn the attention upon them with a sudden and complete awakening; to arrest, to scrutinise them, and to record their exact appearance. Afterwards I collate the records at leisure, and discuss them and draw conclusions. It must be understood that the second of the two ideas was never derived from the first, but always directly from the original object. This was ensured by absolutely withstanding all temptation to reverie. I do not mean that the first idea was of necessity a simple elementary thought: sometimes it was a glance down a familiar line of associations, sometimes it was a well-remem-

bered mental attitude or mode of feeling, but I mean that it was never so far indulged in as to displace the object that had suggested it, from being the primary topic of attention.

I must add, that I found the experiments to be extremely trying and irksome, and that it required much resolution to go through with them, using the scrupulous care they demanded. Nevertheless, the results well repaid the trouble. They gave me an interesting and unexpected view of the number of the operations of the mind, and of the obscure depths in which they took place, of which I had been little conscious before. The general impression they have left upon me is like that which many of us have experienced when the basement of our house happens to be under thorough sanitary repairs, and we realise for the first time the complex system of drains and gas- and water-pipes, flues, bell-wires, and so forth, upon which our comfort depends, but which are usually hidden out of sight, and of whose existence, so long as they acted well, we had never troubled ourselves.

The first experiments I made were imperfect, but sufficient to inspire me with keen interest in the matter, and suggested the form of procedure that I have already partly described. My first experiments were these. On several occasions, but notably on one when I felt myself unusually capable of the kind of effort required, I walked leisurely along Pall Mall, a distance of 450 yards, during which time I scrutinised with attention every successive object that caught my eyes, and I allowed my attention to rest on it until one or two thoughts had arisen through direct association with that object; then I took very brief mental note of them, and passed on to the next object. I never allowed my mind to ramble. The number of objects viewed was, I think, about 300, for I have subsequently repeated the same walk under similar conditions and endeavouring to estimate their number, with that result. It was impossible for me to recal in other than the vaguest way the numerous ideas that had passed through my mind; but of this, at least, I was sure, that samples of my whole life had passed before me, that many bygone incidents, which I never suspected to have formed part of my stock of thoughts, had been glanced at as objects too familiar to awaken the attention.

I saw at once that the brain was vastly more active than I had previously believed it to be, and I was perfectly amazed at the unexpected width of the field of its everyday operations. After an interval of some days, during which I kept my mind from dwelling on my first experiences, in order that it might retain as much freshness as possible for a second experiment, I repeated the walk, and was struck just as much as before by the variety of the ideas that presented themselves, and the number of events to which they referred, about which I had never consciously occupied myself of late years. But my admiration at the activity of the mind was seriously diminished by another observation which I then made, namely that there had been a very great deal of repetition of thought. The actors in my mental stage were indeed very numerous, but by no means so numerous as I had imagined. They now seemed to be something like the actors in theatres where large processions are represented, who march off one side of the stage, and, going round by the back, come on again at the other. I accordingly cast about for means of laying hold of these fleeting thoughts, and, submitting them to statistical analysis, to find out more about their tendency to repetition and other matters, and the method I finally adopted was the one already mentioned. I selected a list of suitable words and wrote them on different small sheets of paper. Taking care to dismiss them from my thoughts when not engaged upon them, and allowing some days to elapse before I began to use them, I laid one of these sheets with all due precautions under a book, but not wholly covered by it, so that when I leant forward I could see one of the words, being previously quite ignorant of what the word would be. Also I held a small chronograph, which I started by pressing a spring the moment the word caught my eye, and which stopped of itself the instant I released the spring; and this I did so soon as about a couple of ideas in direct association with the word had arisen in my mind. I found that I could not manage to recollect more than two ideas with the needed precision, at least not in a general way; but sometimes several ideas occurred so nearly together that I was able to record three or even four of them, while sometimes I only managed one. The second ideas were,

as I have already said, never derived from the first, but always direct from the word itself, for I kept my attention firmly fixed on the word, and the associated ideas were seen only by a half glance. When the two ideas had occurred, I stopped the chronograph and wrote them down, and the time they occupied. I soon got into the way of doing all this in a very methodical and automatic manner, keeping the mind perfectly calm and neutral, but intent and, as it were, at full cock and on hair trigger, before displaying the word. There was no disturbance occasioned by thinking of the imminent revulsion of the mind when the chronograph was stopped. My feeling before stopping it was simply that I had delayed long enough, and this in no way interfered with the free action of the mind. I found no trouble in ensuring the complete fairness of the experiment, by using a number of little precautions, hardly necessary to describe, that practice quickly suggested, but it was a most repugnant and laborious work, and it was only by strong self-control that I went through my schedule according to programme. The list of words that I finally secured was 75 in number, though I began with more. I went through them on four separate occasions, under very different circumstances, in England and abroad, and at intervals of about a month. In no case were the associations governed to any degree worth recording, by remembering what had occurred to me on previous occasions, for I found that the process itself had great influence in discharging the memory of what it had just been engaged in, and I of course took care between the experiments never to let my thoughts revert to the words. The results seem to me to be as trustworthy as any other statistical series that has been collected with equal care.

On throwing these results into a common statistical hotch-pot, I first examined into the rate at which these associated ideas were formed. It took a total time of 660 seconds to form the 505 ideas; that is at about the rate of 50 in a minute or 3000 in an hour. This would be miserably slow work in reverie, or wherever the thought follows the lead of each association that successively presents itself. In the present case, much time was lost in mentally taking the word in, owing to the quiet unobtrusive way in which I found it necessary

to bring it into view, so as not to distract the thoughts. Moreover, a substantive standing by itself is usually the equivalent of too abstract an idea for us to conceive it properly without delay. Thus it is very difficult to get a quick conception of the word "carriage," because there are so many different kinds—two-wheeled, four-wheeled, open and closed, and all of them in so many different possible positions, that the mind possibly hesitates amid an obscure sense of many alternatives that cannot blend together. But limit the idea to, say, a landau, and the mental association declares itself more quickly. Say a landau coming down the street to opposite the door, and an image of many blended landaus that have done so, forms itself without the least hesitation.

Next, I found that my list of 75 words gone over 4 times, had given rise to 505 ideas and 13 cases of puzzle, in which nothing sufficiently definite to note occurred within the brief maximum period of about 4 seconds, that I allowed myself to any single trial. Of these 505, only 289 were different. The precise proportions in which the 505 were distributed in quadruplets, triplets, doublets or singles, is shown in the uppermost lines of Table I. The same facts are given under another form in the lower lines of the table, which show how the 289 different ideas were distributed in cases of fourfold, treble, double, or single occurrences.

TABLE I.

RECURRENT ASSOCIATIONS.

Total number of Associations.	Occurring in quadruplets.	triplets.	doublets.	singles.
505	116	108	114	167
per cent. . 100	23	21	23	33
Total number of different Associations.	**Occurring four times.**	**three times.**	**twice.**	**once.**
289	29	36	57	167
per cent. . 100	10	12	20	58

I was fully prepared to find much iteration in my ideas, but had little expected that out of every hundred words twenty-three would give rise to exactly the same association in every one of the four trials; twenty-one, to the same association in three out of the four, and so on, the experiments having been purposely conducted under very different conditions of time and local circumstances. This shows much less variety in the mental stock of ideas than I had expected, and makes us feel that the roadways of our minds are worn into very deep ruts. I conclude from the proved number of faint and barely conscious thoughts, and from the proved iteration of them, that the mind is perpetually travelling over familiar ways without our memory retaining any impression of its excursions. Its footsteps are so light and fleeting, that it is only by such experiments as I have described that we can learn anything about them. It is apparently always engaged in mumbling over its old stores, and if any one of these is wholly neglected for a while, it is apt to be forgotten, perhaps irrecoverably. It is by no means keen interest and attention when first observing an object, that fixes it in the recollection. We pore over the pages of a 'Bradshaw,' and study the trains for some particular journey with the greatest interest; but the event passes by, and the hours and other facts which we once so eagerly considered become absolutely forgotten. So in games of whist, and in a large number of similar instances. As I understand it, the subject must have a continued living interest in order to retain an abiding-place in the memory. The mind must refer to it frequently, but whether it does so consciously or unconsciously, is not perhaps a matter of much importance. Otherwise, as a general rule, the recollection sinks, and appears to be utterly drowned in the waters of Lethe.

The instances, according to my personal experience, are very rare, and even those are not very satisfactory, in which some event recalls a memory that had lain *absolutely* dormant for many years. In this very series of experiments, a recollection which I thought had entirely lapsed appeared under no less than three different aspects on different occasions. It was this: when I was a boy, my father, who was anxious that I should learn something of physical science, which was then never

taught at school, arranged with the owner of a large chemist's shop to let me dabble at chemistry for a few days in his laboratory. I had not thought of this fact, so far as I was aware, for many years; but in scrutinising the fleeting associations called up by the various words, I traced two mental visual images (an alembic and a particular arrangement of tables and light), and one mental sense of smell (chlorine gas) to that very laboratory. I recognised that these images appeared familiar to me, but I had not thought of their origin. No doubt if some strange conjunction of circumstances had suddenly recalled those three associations at the same time, with perhaps two or three other collateral matters which may still be living in my memory, but which I do not as yet identify, a mental perception of startling vividness would be the result, and I should have falsely imagined that it had supernaturally, as it were, started into life from an entire oblivion extending over many years. Probably many persons would have registered such a case as evidence that things once perceived can never wholly vanish from the recollection, but that in the hour of death, or under some excitement, every event of a past life may reappear. To this view I entirely dissent. Forgetfulness appears absolute in the vast majority of cases, and our supposed recollections of a past life are, I believe, no more than that of a large number of episodes in it, to be reckoned in hundreds or thousands, certainly not in tens of hundreds of thousands, which have escaped oblivion. Every one of the fleeting, half-conscious thoughts which were the subject of my experiments admitted of being vivified by keen attention, or by some appropriate association; but I strongly suspect that ideas which have long since ceased to fleet through the brain, owing to the absence of current associations to call them up, disappear wholly. A comparison of old memories with a newly-met friend of one's boyhood, about the events we then witnessed together, shows how much we had each of us forgotten. Our recollections do not tally. Actors and incidents that seem to have been of primary importance in those events to the one, have been utterly forgotten by the other. The recollection of our earlier years are, in truth, very scanty, as any one will find who tries to enumerate them.

My associated ideas were for the most part due to my own unshared experiences, and the list of them would necessarily differ widely from that which another person would draw up who might repeat my experiments. Therefore one sees clearly, and I may say, one can see *measurably*, how impossible it is in a general way for two grown-up persons to lay their minds side by side together in perfect accord. The same sentence cannot produce precisely the same effect on both, and the first quick impressions that any given word in it may convey, will differ widely in the two minds.

I took pains to determine as far as feasible the dates of my life at which each of the associated ideas was first attached to the word. There were 124 cases in which identification was satisfactory, and they were distributed as in Table II.

TABLE II.

RELATIVE NUMBER OF ASSOCIATIONS FORMED AT DIFFERENT PERIODS OF LIFE.

Total number of different Associations.		Occurring four times.		three times.		twice.		once.		Whose first formation was in
	per cent.		per cent.		per cent.		per cent.		per cent.	
48	39	12	10	11	9	9	7	16	13	boyhood and youth,
57	46	10	8	8	7	6	5	33	26	subsequent manhood,
19	15	—	—	4	3	1	1	14	11	quite recent events.
124	100	22	18	23	19	16	13	63	50	Totals.

It will be seen from the table that out of the 48 earliest associations no less than 12, or one quarter of them occurred in each of the four trials; of the 57 associations first formed in manhood, 10, or about one-sixth of them had a similar recurrence, but as to the 19 other associations first formed in quite recent times, not one of them occurred in the whole of the four trials. Hence we may see the greater fixity of the earlier associations, and might measurably determine the decrease of fixity as the date of their first formation becomes less remote.

The largeness of the number 33 in the fourth column, which

disconcerts the run of the series, is wholly due to a visual memory of places seen in manhood. I will not speak about this now, as I shall have to refer to it further on. Neglecting, for the moment, this unique class of occurrences, it will be seen that one-half of the associations date from the period of life before leaving college; and it may easily be imagined that many of these refer to common events in an English education. Nay further, on looking through the list of all the associations it was easy to see how they are pervaded by purely English ideas, and especially such as are prevalent in that stratum of English society in which I was born and bred, and have subsequently lived. In illustration of this, I may mention an anecdote of a matter which greatly impressed me at the time. I was staying in a country house with a very pleasant party of young and old, including persons whose education and versatility were certainly not below the social average. One evening we played at a round game, which consisted in each of us drawing as absurd a scrawl as he or she could, representing some historical event; the pictures were then shuffled and passed successively from hand to hand, every one writing down independently their interpretation of the picture, as to what the historical event was that the artist intended to depict by the scrawl. I was astonished at the sameness of our ideas. Cases like Canute and the waves, the Babes in the Tower, and the like, were drawn by two and even three persons at the same time, quite independently of one another, showing how narrowly we are bound by the fetters of our early education. If the figures in the above table may be accepted as fairly correct for the world generally, it shows, still in a measurable degree, the large effect of early education in fixing our associations. It will of course be understood that I make no absurd profession of being able by these very few experiments to lay down statistical constants of universal application, but that my principal object is to show that a large class of mental phenomena, that have hitherto been too vague to lay hold of, admit of being caught by the firm grip of genuine statistical inquiry.

The results that I have thus far given are hotch-potch results. It is necessary to sort the materials somewhat, before saying more about them.

After several trials, I found that the associated ideas admitted of being divided into three main groups. First there is the imagined sound of words, as in verbal quotations or names of persons. This was frequently a mere parrot-like memory which acted instantaneously and in a meaningless way, just as a machine might act. In the next group there was every other kind of sense-imagery; the chime of imagined bells, the shiver of remembered cold, the scent of some particular locality, and, much more frequently than all the rest put together, visual imagery. The last of the three groups contains what I will venture, for want of a better name, to call "histrionic" representations. It includes those cases where I either act a part in imagination, or see in imagination a part acted, or, most commonly by far, where I am both spectator and all the actors at once, in an imaginary mental theatre. Thus I feel a nascent sense of some muscular action while I simultaneously witness a puppet of my brain—a part of myself—perform that action, and I assume a mental attitude appropriate to the occasion. This, in my case, is a very frequent way of generalising, indeed I rarely feel that I have secure hold of a general idea until I have translated it somehow into this form. Thus the word "abasement" presented itself to me, in one of my experiments, by my mentally placing myself in a pantomimic attitude of humiliation with half-closed eyes, bowed back, and uplifted palms, while at the same time I was aware of myself as of a mental puppet, in that position. This same word will serve to illustrate the other groups also. It so happened in connection with "abasement" that the word "David" or "King David" occurred to me on one occasion in each of three out of the four trials; also that an accidental misreading, or perhaps the merely punning association of the words "a basement," brought up on all four occasions the image of the foundations of a house that the builders had begun upon.

So much for the character of the association; next as to that of the words. I found, after the experiments were over, that the words were divisible into three distinct groups. The first contained "abbey," "aborigines," "abyss," and others that admitted of being presented under some mental image.

The second group contained "abasement," "abhorrence," "ablution," &c., which admitted excellently of histrionic representation. The third group contained the more abstract words, such as "afternoon," "ability," "abnormal," which were variously and imperfectly dealt with by my mind. I give the results in the upper part of Table III., and, in order to save trouble, I have reduced them to percentages in the lower lines of the table.

TABLE III.

COMPARISON BETWEEN THE QUALITY OF THE WORDS AND THAT OF THE IDEAS IN IMMEDIATE ASSOCIATION WITH THEM.

Number of words in each series.		Sense imagery.	Histrionic.	Purely names of persons.	Verbal phrases and quotations.	Total.
26	"Abbey" series	46	12	32	17	107
20	"Abasement" „	25	26	11	17	79
29	"Afternoon" „	23	27	16	38	104
75						290
	"Abbey" series	43	11	30	16	100
	"Abasement" „	32	33	13	22	100
	"Afternoon" „	22	25	16	37	100

We see from this that the associations of the "abbey" series are nearly half of them in sense imagery, and these were almost always visual. The names of persons also more frequently occurred in this series than in any other. It will be recollected that in Table II. I drew attention to the exceptionally large number, 33, in the last column. It was perhaps 20 in excess of what would have been expected from the general run of the other figures. This was wholly due to visual imagery of scenes with which I was first acquainted after reaching manhood, and shows, I think, that the scenes of childhood and youth, though vividly impressed on the memory, are by no means numerous, and may be quite thrown into the background by the abundance of after experiences; but this, as we have seen, is not the case with

the other forms of association. Verbal memories of old date, such as Biblical scraps, family expressions, bits of poetry, and the like, are very numerous, and rise to the thoughts so quickly, whenever anything suggests them, that they commonly outstrip all competitors. Associations connected with the "abasement" series are strongly characterised by histrionic ideas, and by sense-imagery, which to a great degree merges into a histrionic character. Thus the word "abhorrence" suggested to me, on three out of the four trials, an image of the attitude of Martha in the famous picture of the raising of Lazarus by Sebastian del Piombo in the National Gallery. She stands with averted head, doubly sheltering her face by her hands from even a sidelong view of the opened grave. Now I could not be sure how far I saw the picture as such, in my mental view, or how far I had thrown my own personality into the picture and was acting it as actors might act a mystery play, by the puppets of my own brain, that were parts of myself. As a matter of fact, I entered it under the heading of sense-imagery, but it might very properly have gone to swell the number of the histrionic entries.

The "afternoon" series suggested a great preponderance of mere catch-words, showing how slowly I was able to realise the meaning of abstractions; the phrases intruded themselves before the thoughts became defined. It occasionally occurred that I puzzled wholly over a word, and made no entry at all; in thirteen cases either this happened, or else after one idea had occurred the second was too confused and obscure to admit of record, and mention of it had to be omitted in the foregoing table. These entries have forcibly shown to me the great imperfection in my generalising powers; and I am sure that most persons would find the same if they made similar trials. Nothing is a surer sign of high intellectual capacity than the power of quickly seizing and easily manipulating ideas of a very abstract nature. Commonly we grasp them very imperfectly, and hold on to their skirts with great difficulty.

In comparing the order in which the ideas presented themselves, I find that a decided precedence is assumed by the Histrionic ideas, wherever they occur; that Verbal associations

occur first and with great quickness on many occasions, but on the whole that they are only a little more likely to occur first than second; and that Imagery is decidedly more likely to be the second, than the first, of the associations called up by a word. In short, gesture-language appeals the most quickly to our feelings.

It would be very instructive to print the actual records at length, made by many experimenters, if the records could be clubbed together and thrown into a statistical form; but it would be too absurd to print one's own singly. They lay bare the foundations of a man's thoughts with curious distinctness, and exhibit his mental anatomy with more vividness and truth than he would probably care to publish to the world.

It remains to summarise what has been said in the foregoing memoir. I have desired to show how whole strata of mental operations that have lapsed out of ordinary consciousness, admit of being dragged into light, recorded and treated statistically, and how the obscurity that attends the initial steps of our thoughts can thus be pierced and dissipated. I then showed measurably the rate at which associations sprung up, their character, the date of their first formation, their tendency to recurrence, and their relative precedence. Also I gave an instance showing how the phenomenon of a long-forgotten scene, suddenly starting into consciousness, admitted in many cases of being explained. Perhaps the strongest of the impressions left by these experiments regards the multifariousness of the work done by the mind in a state of half-unconsciousness, and the valid reason they afford for believing in the existence of still deeper strata of mental operations, sunk wholly below the level of consciousness, which may account for such mental phenomena as cannot otherwise be explained. We gain an insight by these experiments into the marvellous number and nimbleness of our mental associations, and we also learn that they are very far indeed from being infinite in their variety. We find that our working stock of ideas is narrowly limited, but that the mind continually recurs to them in conducting its operations, therefore its tracks necessarily become more defined and its flexibility diminished as age advances.

ON THE ELECTRICAL EXCITABILITY OF THE SKIN.

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THE object of the following pages is threefold. First, to indicate the conditions which any rational method must fulfil which pretends to give us accurate results as to the electrical excitability of the cutaneous nerves; these conditions will be found to depend partly upon the behaviour of the electrical current, partly upon certain anatomical peculiarities of the skin. Secondly, to criticise in the light of the principles set forth the chief methods hitherto proposed to estimate the electrical sensibility. Third, to propose a new method more simple and rational; and state some of the results already obtained from its application.

I.

According to the general law of excitation of nerves, formulated by Du Bois-Reymond, this excitation depends upon the *rapidity* with which the electrical density changes in the nerve, and not with the *absolute value* of that change.

Hence we ought, for measuring the electrical excitability of a nerve, to determine the rapidity of change in the density necessary to excite it; and this is what Dr. v. Fleischl's[1] ingenious instrument, the Rheonome, enables us to do. Only

[1] "Untersuchung über die Gesetze der Nervenerregung," III. Abhandlung. Stzber. d. k. Academie der Wissensch. zu Wien. Vol. lxxvi., Part III., p. 138.

such measurements are much too complex to be of any practical clinical value. It is, however, possible to attain the same object by simpler means, through the elimination of some of the variables in the experiment. Thus if the current is made through the nerve with always the same rapidity, so that it reaches its maximum intensity in the same space of time, we may measure the excitability of the nerve by the density of the current flowing through it. Or again, if the diameter of the nerve remains constant, its excitability may be estimated by the strength (intensity) of the current.

It is easy to base upon these considerations a rational method for measuring the electrical excitability of the skin. This would consist in giving the galvanometric value of the currents necessary to produce the minimum sensation in every part of the skin; provided that these two conditions are observed: (1) the make and break of the current must be made every time with the same rapidity; (2) the surface of contact of the electrode with the skin must always be the same. It is evident that by using this method we are independent of all the variations in the resistance, permanent or incidental, originating in the parts to be examined. Let it be clearly understood, however, that we assume here a direct galvanometric measurement of the current strength, and not an estimation of it by resistances introduced either in the circuit itself, or in a derived current.[1]

Given two points of the skin of different electrical resistance; we must, to obtain the same current-strength in both cases, use a different number of elements. Once the same current-strength obtained, the excitation is the same in both cases, provided always the other conditions be fulfilled, i.e. equal rapidity of make and break, and equal extent of excited surface. Now it is clear that under such circumstances, if the excitability be the same, the effect of the excitation must be the same also.

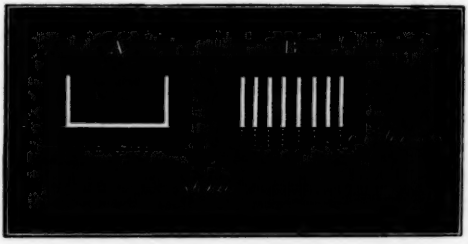
By these means, then, we are made independent of the source of error pointed out frequently before and arising from

[1] The latter process, so often employed by electrotherapeutists, after the example of Brenner, ought to be entirely abandoned. Applied in researches such as those we have described, for instance, it would give entirely erroneous results.

the variable thickness of the epidermis. Yet, in order to be able to measure the excitability, not of the collective nervous elements of the skin, but of the cutaneous nerves themselves, we must be able to eliminate the sources of variation arising from the structural differences of the nervous supply in different parts of the skin.

We have, it is true, no direct anatomical demonstration of such differences; but physiological observations, such as the measurements with E. H. Weber's compasses, allow us to suppose that the number of nervous endings in a unit of cutaneous surface varies in different parts of the body. If this supposition be true, it necessarily follows that an electrode of the same surface applied to different parts of the body ought to produce excitations of various strengths merely on account of the variable richness of the skin in nerve-endings.

The way to eliminate this source of error consists in always exciting the skin at the same number of points by means of a special form of electrode. The simplest would be a pointed electrode which would eliminate the influence of a variable number of excitations by reducing this number to a minimum; but its sharpness would make an accurate estimation of the minimal electrical excitation difficult. The following form of electrode, however, satisfies the present condition sufficiently well: It consists in a small cylindrical bundle of well insulated wires; and it is not difficult to understand the important difference between the effects of such an electrode and those of one in the shape of a solid cylinder of the same diameter.



The figure makes this difference evident. The excitation produced by B will be stronger than that produced by A

because the surface of the metallic contact with the skin is less, and hence the density of the current greater. Again, and this is the important point, the number of the excitations in the case of B will be more uniformly the same, whilst in the case of A it will depend upon the abundance of cutaneous nerve-endings. For it may be assumed that, with B, none but the shortest currents from the wires through the epidermis will stimulate the nerves; that is, none but the nerve-fibres lying nearest to the wires (the points of greatest electrical density) will be stimulated—especially as we here deal merely with minimal excitations. We shall see further on that experience has completely borne out these theoretical considerations.

There is yet a condition that has not hitherto been attended to in testing the sensibility of the skin, and which must however, have some influence on the results of the experiments. One of us (Dr. Tschiriew) in a paper forthcoming in the *Archives de Physiologie* (Charcot, Vulpian, and Brown-Séquard) starting from physiological and pathological data, shows that it is necessary to assume the interruption of all the centripetal paths in the grey matter of the spinal cord; and that the differences in the duration and intensity of the excitation necessary to produce the same effect may be explained by the variations occurring in that interruption among the several afferent nerves. Hence it follows that, in order to eliminate differences due to any such variations possible among the afferent cutaneous nerves, it is necessary to allow a certain duration to each excitation.

In order to fulfil this condition if the continuous current were used, and also to avoid the mechanical and electrotonic influences of this current upon the tissues, we should require an apparatus which would interrupt the current, and at the same time reverse it each time, at a uniform rate of speed. This obviously complicates the process so much as to make it inapplicable for clinical observation. But before we pass to the description of a simpler and more practical method we must say a few words on those hitherto proposed, and examine how far they fulfil the conditions just laid down.

II.

In 1864 Munk and Leyden ('Untersuchungen über die Sensibilität im gesunden und kranken Zustande,' Virchow's 'Archiv,' vol. xxxi. page 1) described the following method for testing the electro-cutaneous sensibility. A pair of wooden compasses were fitted with metallic terminations connected with the secondary coil of a Du Bois-Reymond's induction apparatus. The points were fixed at a constant distance from one another (1 cm.), and applied to the skin. The sensibility was then determined by noting the distance between the two coils when the point of minimum sensation was reached. Simple make or break induced currents, of constantly the same rapidity, were used. This method led its authors to the following results: (1) Different regions of the skin differ in their absolute sensibility in much the same degree as in their sense of space (Weber).[1] (2) The electro-cutaneous sensibility decreases in the following order: face, trunk, upper arm and thigh, elbow and knee, tips of fingers and toes.

The authors themselves define the value of these measurements by adding that the local thickness of the epidermis, as well as the local abundance of nerve fibres, must influence the results obtained. This method is indeed faultless if our object is merely to determine the electro-sensibility of the skin as a whole, with all its incidental peculiarities, such as the varying thickness of its epidermis, abundance of its sensitive elements, conductivity of its subjacent structures, and mode of interruption of its afferent nerves in the spinal cord. For it is admissible that in every healthy individual the *distribution* of these anatomical peculiarities of the skin in its different parts follows the same law. On the other hand it would be impossible to draw any conclusions whatever from results obtained by this method about the sensibility of the cutaneous nervous elements themselves, since these results have been obviously modified by the various conditions just enumerated.

[1] This hardly agrees with the results obtained by the authors themselves: the tips of the fingers, for instance, are more developed for the sense of space than the trunk.

Bernhardt ('Electrotherapeutische Notizen,' Deutsches Archiv für Klinische Medicin, 1877, vol. xix. p. 382), whilst objecting to the foregoing, the practical difficulties in the comparison of results obtained with different induction apparatus, proposes the following method: to measure the electrocutaneous sensibility by the resistances which must be intercalated in a derived circuit in order to reach the minimum sense of pain. The "modus operandi" is as follows: The patient holds the positive electrode of a battery of thirty Stöhrer's elements in his left hand. The sensibility is tested by means of a wire brush connected with the negative pole. A derived current is established through a rheostat, and the resistances read off as soon as pain is experienced. These readings give the measure of the sensibility.

This method offers no advantage over that of Munk-Leyden; the results obtained even for the same individual are hardly comparable among themselves. The relations between the variations in the rheostat resistances and the current strengths thus set up through the body, are obviously far more complex than those between the distances of the coils and the strength of the induced currents in a non-graduated apparatus. Besides, given the number of cells included in the inducing circuit, and the number of turns of wire in both coils, the true value of the distances between the two coils noted in any experiment can at any time be ascertained; whereas the value of the rheostat resistances could be determined only by the repetition of all the measurements upon the same person, and by a process the complexity of which is in itself greater than the graduation of an induction apparatus.

Again the determination of the minimum sensation obtained on exciting by a single make or break of a continuous current, owing either to the longer time it takes to attain its maximum intensity or to the electrotonic influences it exerts upon the tissues, is much less precise than that produced by the induced current. This is easy to demonstrate upon oneself. The matter becomes of still greater importance when we have to do with patients whose intelligence is not always of the keenest. We may also recall the well-known phenomenon explainable by the electrotonic effects of the galvanic current:

that, when we think we have reached the minimum current strength that will produce sensation, we find, on controlling the experiment by further diminishing the current, sensation will now manifest itself to much weaker stimulations than at first.

But the greatest objection to Bernhardt's method is the fact that the resistance of the body included in the circuit varies with every new position of the negative electrode—a source of errror which he gives us no means to remedy. As an instance of the results he obtains we may quote the following measurements: for the tip of the nose, from 50 to 60 Siemens' units,[1] intercalated resistance sufficed; whereas 2000 to 3000 S. U. were necessary for the tips of the fingers and toes! As to the general conclusions reached by the author, they agree with those of Munk and Leyden. He, too, refrains from drawing any as to the irritability of the cutaneous nerves themselves.

The current number of the 'Archiv für Psychiatrie' contains a paper by Drosdoff ('Untersuchungen über die electrische Reizbarkeit der Haut bei Gesunden und Kranken,' vol. ix. part 2). The highly elaborate form in which these researches are presented, compels us to pay more attention to them than their intrinsic value deserves. Some of the objections raised by the writer to the previous methods are either utterly groundless or have been anticipated by their authors themselves. Thus, for instance, when he objects that the results obtained by Munk and Leyden are vitiated by the varying abundance of nerve elements in the skin, he forgets that, so far from falling into such an error, these authors had pointed out this fact as forbidding them to conclude from the sensibility of "the skin" to that of "the cutaneous nerves." This does not prevent Dr. Drosdoff from falling into it himself, though judged from his own standpoint, the method he uses is still more defective than that of the above-named authors. Like Bernhardt he fixes one of the electrodes on some part of the body (the sternum), and explores the sensibility by means of the other electrode in the shape of a wire brush. He uses a non-graduated induction apparatus, and estimates the sensibility by the distance

[1] Siemens' unit of resistance is equal to ·97 ohm nearly.

between the two coils, like Munk and Leyden. This method is thus merely a combination of the two former ones, from the imperfections of which it does not escape. It unites the limited applicability of the first (Munk-Leyden) to the weakest point of the second (Bernhardt), in that it introduces a different resistance into the circuit, with every change of position of the exciting electrode. Neither do the results obtained by Drosdoff differ materially from those of the previous observers. In order, however, to find out whether the differences in the sensibility discovered in different parts of the body are not simply due to differences in the resistance of these parts, the author has made numerous measurements of what he calls "epidermic resistances." For this purpose he fixes one of the electrodes of a battery of twelve Stöhrer's elements on the sternum, and applies the other to the various parts of the body, the sensibility of which has been tested. A galvanometer with 150 turns of wire is introduced in the circuit, by the readings of which the resistances are estimated. Now it is evident that it was the resistance of the whole portion of the body included between the electrodes and not that of the epidermis that the author was measuring. Again, if the instrument used was a simple sinus-galvanometer, and was not graduated in absolute units—and he says not a word on this important point—it is evident that his measurements did not correspond to the absolute value of the electrical forces. As it is, one is struck at first sight by the parallelism between the series of numbers obtained by the resistance-measurements and those obtained by the sensibility-measurements at different points of the body; but, strange to say, the author draws from them the following conclusion: that there is no relation between the differences of resistance and of sensibility of the same points. Let the reader judge for himself. At page 213, under the title of "electrical zones," we find the following numbers:

M.S. . .	232·5	212·0	200·7	193·3	154·2	188·1
M.P. . .	165·2	156·5	146·4	142·3	123·6	142·3
Dev. . .	22·2°	9·3°	6·9°	4·5°	2·5°	6·5°

And at page 215:

M.S. . .	184·6	184·2	178·1	138·8	
M.P. . .	143·0	140·0	133·0	117·6	
Dev. . .	7·1°	5·3°	3·4°	2·9°	

M.S. stands for minimum sensation; M.P. for minimum pain; Dev. for deviation in degrees of the needle. Each vertical column of numbers corresponds to one of the author's zones. These numbers are means of ten observations, and therefore have a higher intrinsic value than each of their component factors. The parallelism which we observe among them is quite as marked as what we could expect from the imperfect method used and the peculiarities of human bodies.

The way Dr. Drosdoff escapes the conclusions forced upon him by his own data is to pick out among the individual factors a few exceptional numbers in which the parallelism does not exist; and from these exceptions, due probably to unavoidable errors of observation, he concludes that there is no such thing as a relation between the variations of the resistance and of the sensibility. Further, upon this extraordinary conclusion, the author bases another assertion of still higher import, and which he gives as the grand result of his investigation, viz.: "that the differences of sensibility at different points of the skin are due to differences in the excitability of the nerves themselves." We shall state evidence further on to disprove the author's conclusions, from the results of our own experiments. They are far from being deducible from his own measurements, and indeed it would seem that he has conspicuously brought into evidence the great influence variations of resistance have exerted upon the results obtained. Elsewhere he is led astray by his ignorance of the properties of the instruments he used. He says (page 219), "The difference ('Verhältniss') between the minima of sensation and those of pain varies between 6 and 88 mm. (mean: 11–73.6) distance between the two coils. These distances diminish, in the case of painful impressions, with the increase of current strength." This statement is false if by it is meant that there

is any relation between the two kinds of sensation; for the author's results have been obtained with a non-graduated apparatus, and is ascribable to the mere absence of proportionality between the distances of the coils and the strengths of the corresponding currents.

III.

From what we have said, it is clear that the method of Munk-Leyden is sufficient for measuring the sensibility of the skin; and that none of the methods hitherto proposed enables us to test the electrical excitability, absolute or relative, of the cutaneous nerves themselves.

We have pointed out previously a rational method for accomplishing this object. But, as we saw, though correct, it is too complex to be applicable to clinical purposes. We have, however, another one to propose, which is simple, though fulfilling all the conditions which have been laid down previously. We now pass to the description of this method, and of the results we have been able to obtain by its application.

Its principles are the following: (1) Elimination of all the sources of variation in the strength of the currents due to the variable thickness of the epidermis,[1] and the different positions of the electrodes, &c., by intercalating in the circuit such resistances as to make such variations insignificant; (2) Elimination of the influence of the variable abundance of nervous elements in the skin by exciting it at a constant number of points disposed over a constant surface. The latter is effected by using the form of electrode described previously, and composed of a solid cylinder of insulated wires.

In order to fulfil the first condition, it was necessary to know what resistances the human body could offer in such experiments. The measurement of these resistances is far from being such a simple process as is commonly thought. The conditions of experimentation from which they are ob-

[1] This condition has often been held up as very unfavourable to the appreciation of the sensibility of the cutaneous nerves, and it is thought that were that thickness known at various parts of the body much could be gained thereby. We hold the opposite view; for if we cannot eliminate by the method itself the influence of the epidermis, how can we hope to be able to do so by calculation when we are acquainted, ever so precisely, with its thickness at every part of the body?

tainable are so various as to make it impossible to speak absolutely of the electrical resistance of a part of the body, but only of its resistance under this, that, or the other condition. For instance, according to the details of the process of experimentation adopted, the "resistance of the forearm" may vary from about two to forty (or more) thousand ohms. The main circumstances influencing the results are: (1) the size, shape, and moisture of the electrodes; (2) the pressure with which they are applied; (3) the strength of the currents employed; (4) the condition of the parts tested, e.g. the degree moisture of the epidermis, the previous application of a more or less strong current, &c.

The first circumstance is of easy explanation. The fact that resistance diminishes with the pressure exerted upon the electrode is not so satisfactorily accounted for. It is more readily observed in parts of the skin with a compressible, rather than a hard, subjacent tissue. The electrical resistance may be diminished, on increasing the pressure, by one-half, two-thirds, or even more.

The variations dependent upon the current strength are illustrated by the well-known fact that when first applied to the skin a current from a large number of elements may be weak, but soon becomes stronger, showing that the patient's resistance diminishes. This diminution must be, partially at least, explained by the cataphorical effect of the current (du Bois-Reymond) by which liquids are conveyed into the integuments either from the electrode or from the subjacent tissues. Hence weak currents must be used for measurements in order to eliminate this source of error. The fourth condition referred to above may also be reduced to the increased moisture of the integuments. The following experiment will illustrate it. An electrode is fixed to the back, and another applied to the dorsal surface of the forearm. The resistance is found to be about 10,000 ohms. After a little rubbing of the arm with the electrode, and a strong current, the resistance is found to have sunk below 3000 ohms. The practical outcome of all this for measurements of body-resistances is that, first, all the conditions of observation must remain the same throughout; second, all these conditions must be carefully described in

every case. If these requisites are not fulfilled, the data obtained cannot have the slightest value.

Our measurements were taken as follows: A metallic plate, 5 × 12 cm., covered with wet wash-leather, was fixed to the upper part of the back; the various parts of the body were explored with a metallic disk 7 mm. in diameter, also enclosed in wet wash-leather. The latter electrode was fixed to an Eulenburg's baræsthesiometer, in order to ensure the same amount of pressure everywhere. The pressure used was of 150 grammes. The skin at the point of application was moistened, and the time taken for each observation restricted to a few seconds, in order to avoid differences arising to the unequal imbibition of the epidermis. Several observations were taken for each region, but the electrode was never re-applied to exactly the same spot, or only so after some time had elapsed. The battery used was a Gaiffe-Leclanché, and the number of elements used (4-14) chosen so as to obtain at every part a weak current (.2 to .5 milliveber),[1] and to avoid the disturbing effect of strong currents on the tissues. The following table gives our own resistances in ohms:

	Ohms.	Ohms.
Tip of nose	12,000	8,100
Forehead	4,000	3,000
Cheek	8,200	6,400
Forearm (post)	18,750	14,000
„ (anter.)	20,000	..
Hand (dorsal)	21,000	15,500
„ (palmar)	42,000	48,000
Tips of fingers	65,000	46,000
Leg	21,000	23,000
Foot (dorsum)	22,000	24,600
„ (sole)	80,000	50,000
Tips of toes	60,000	60,000

[1] A current of one milliveber is that obtained with one volt through one thousand ohms. The electromotive force of a Daniell's cell is nearly one volt.

These numbers hold only for limited areas of the regions investigated. The different portions of the leg, for instance, tested under the same conditions throughout will vary in resistance according to the peculiarities of the epidermis and subjacent tissues. Similar measurements made with the electrode used in testing the sensibility (a cylindrical fasciculus of insulated wires) gave still more considerable differences, especially with the dry skin. In this way the resistances of the tips of the fingers and toes may amount to 100,000 ohms and more.

Hence we see that in order fully to eliminate the influence of the body-resistance in investigating the sensibility on Bernhardt's principle, at least a 2-million ohms' resistance is to be introduced in the circuit; and it is easy to perceive how much this author's and Drosdoff's results must have been influenced by these sources of fallacy which they have ignored.

For the investigation of the sensibility, our method consisted in fixing a large neutral plate-electrode on the back; and in using as differential or exciting electrode a metallic wire brush, of which the wires were insulated with sealing-wax, and brought together to form a cylindrical bundle of 75 mm. diameter. Care was taken that the exciting surface was as smooth as possible. This electrode was mounted on an interrupting handle. A Du Bois-Reymond's induction apparatus, fed by two Bunsen's cells, was used. The secondary coil (600 metres of 0·225 mm. copper wire) was used, and in the circuit included a resistance of upwards of three million ohms, formed by a slip of vulcanite overlaid with a film of plumbago. The hammer of the apparatus was made to vibrate at a rapid rate, and the current closed by means of the key through the previously applied electrode. This is a most important precaution, as "dabbing" the skin with the current on would produce such variations in the density of the current as to vitiate all the results. The minimum point of sensation was sought by adapting the distance between the two coils, whilst excitations were made by making and breaking the current by means of the key, the electrode remaining immoveable *in situ*. Finally, sufficient time was allowed for each excitation.

In this way we have arrived at very unexpected results.

In opposition to what has been stated by previous observers, we have found that *the electrical excitability of the skin, or rather of the nerves of the skin, is the same at every part of the body.* There always occur slight differences (amounting to from a few mm. to 1 cm. of coil); but on the one hand these differences are not always in the same sense, on the other their absolute value is too small [1] to claim any special signification.

Several points must be attended to in order to obtain reliable data with our method. (1) The elements feeding the coil must be *constant* (this excludes Leclanché's, and all single fluid cells, such as Stöhrer's, Smee's, &c.). We used two Bunsen's. (2) The conducting wires must be thoroughly insulated from every surrounding object, owing to the enormous "tension" [2] of the current. (3) As before mentioned, the excitation must begin after the electrode is *in situ.* (4) The portion of skin tested must be moistened. At first sight this last condition appears superfluous; for how, it is asked, can the hygrometric state of the skin have any influence when the circuit contains already such an enormous resistance?

We venture to offer the following explanation of this phenomenon. It is not due to any change in the resistance of the skin; because such a change would, as objected, be of no appreciable influence under the circumstances, and because also—a fact opposed to such a supposition—dry skin is more excitable than moist skin. But, as it has been experimentally shown (Tschiriew, 'Ueber die Nerven- und Muskelerregbarkeit,' in Du Bois-Reymond's Archiv f. Anat. & Phys., 1878, p. 494), electricity distributes itself, in a transverse section of every conductor it passes through, always in an inverse ratio to the resistance (Kirchoff's law), *independently of any resistance in the circuit.*

[1] Though the curve representing the relation between the distances of the two coils and the current-strengths corresponding to these distances is entirely independent of the resistances in the circuit of the secondary coil, the absolute value of these currents is *not* independent of these resistances, which appear in the curve as one of the parameters. Since then, in our experiments where the circuit included an intercalated resistance of several million ohms, the variations of 1 cm. or less in the coil-distances can correspond but to very small variations in the current-strengths.

[2] De Watteville, "On the Nature of Electrical Tension."—'Medical Times and Gazette,' Sept. 1877, and 'Med. Electricity,' Chap. I.

Hence even in our experiments where several million ohms' resistance was included in the circuit, even in the case of the dry skin where we must assume differences of resistance between the dry epidermis and the sudoriparous ducts, the whole current would find its way through the points of least resistance. Hence a current, which on a moist skin would have been hardly felt, may become even painful on a dry one. It is a well-known fact that the excitation of the dry skin produces a pungent sensation; of the moist skin a milder sensation—the latter being more evenly diffused than the former. Again, this difference is less marked in some regions than in others; for instance, less at the finger-tips than on the cheek or dorsum of hand. Hence appears the importance of eliminating this source of fallacy.

Another capital precaution in sensibility-testing is to avoid placing the electrode on any nervous twig. The peculiar sensation evoked will tell us if it has been done; and as a rule we must always choose the least sensitive spots for exciting. Generally speaking it will be advisable to avoid those spots where the epidermis is very thick; this latter condition would involve not only a higher resistance, but also a greater thickness of the layer interposed between the nervous elements and the electrode, and thus increase the diffusion of the currents before they reach the nerve-endings. In order to illustrate the influence of the kind of electrode we use (cylindrical bundle of insulated wires) in eliminating the influence of the variable abundance of cutaneous nerve-elements, we have compared its effects with those of a solid cylinder of the same diameter (7 mm.). With the former, as above related, we found the excitability of every part of the body practically the same. With the latter, we found it different, and to a certain extent varying in the same ratio as Weber's sense of space. Thus:

	cm.		cm.
Nose	7·	Hand (palm)	5·
Forehead	7·2	Tips of fingers	8·3
Lips	7·3	Leg	3·
„ (red part)	5·3	Foot (dorsum)	4·
Forearm (front)	6·6	„ (sole)	0·
„ (back)	5·2	Tips of toes	7·5
Hand (back)	5·5		

In the interpretation of these data, however, it must be noted that we used a non-graduated induction apparatus, fed by two Leclanché's, with several million ohms' resistance in the circuit. We have seen before that the absolute strength of induced currents diminishes in an inverse ratio to the intercalated resistance. Hence the absolute value of the excitations corresponding to the coil-distances just given is much less than if the body alone had been included in the circuit.

The different results obtained from the use of the two forms of electrodes can be explained only by the fact of the varying nervous supply of different regions of the skin, and the elimination of its influence in the case of the special electrode.

Another observation we have made in the course of our experiments and which illustrates the importance of the shape of the electrode in exciting the skin, is, that if the bundle of insulated wires was made into an elliptical instead of a cylindrical column, the results obtained depended upon the relative position of its long axis with reference to the prevailing direction of the subcutaneous nerves. If the long axis coincides with this direction (as for instance in the limbs in the direction of their length) the excitation produced is less powerful than when it is transverse to the general course of the nerves. The same result is observable when a solid elliptical electrode is used.

This fact does not disprove our assertion about the possibility of eliminating the influence of the relative abundance of nerve-elements in different parts of the skin by our electrode. For, besides the arguments derived from comparative experiments, it is hardly possible to conceive such a regular distribution of nerve-endings in the skin that a mere alteration in the direction of the electrode should be followed by a change in the number of the endings influenced. We must assume, then, that we have here to do with a new factor of influence in the measurement of sensibility, viz., the mode of division and subdivision of the subcutaneous nerves.

An analogous phenomenon is observed with Weber's compasses. If at the same spot the minimum distance of distinct

impressions is sought in various directions, it is found (especially in the limbs) that these distances are greater in a parallel than in a transverse direction to the main course of subcutaneous nerves. Hence Weber's "circles" are ellipsoids with their long diameter directed along those nerves.

The explanation of this phenomenon is, that when the long axis of the electrode coincides with the main direction of the nerve-trunks, the larger number of exciting points influence the terminations of the same system of ultimate fibres of some one nervous twig. If, on the other hand, the long axis of the electrode is directed transversely to the nerves, the number of excited points of each twig will be less, but the number of those twigs stimulated will be greater.

Hence it will be observed that for testing the sensibility, electrodes with a circular surface of contact are alone to be used in order to eliminate this disturbing factor from the results.

The advantages of the method we have adopted are, that it is simple, easily applicable in clinical investigations,[1] and that its results, to be understood, do not require any comparison with tables of the distribution of sensibility in different parts of the body. As an example of its application we subjoin the results obtained in a case of bulbar paralysis with lateral sclerosis. All the tendon-reflexes were exaggerated, especially on the right side. The sense of touch was nowhere greatly impaired, but there was decided diminution of the sensation to pain, on the right side especially.

[1] In order to facilitate the application of our method, we are endeavouring to produce an appropriate electrode. Our present model is to consist of a tube of non-conducting material containing in its upper part a rod of resisting substance —kaolin and graphite—of two million ohms; in its lower, a cylinder of metal, the extremity of which forms the exciting surface. This surface will be subdivided by a system of intersecting grooves filled with an insulating substance, into a number of exciting points, as shown in the diagram already figured. This little appliance, mounted upon an ordinary interrupting handle, will be all that is required, besides an induction apparatus with a moderately long and fine secondary wire and a constant element.

Part of Body.	Distances of Coils.	
	Right.	Left.
Nose	6·	7·2
Forehead	6·1	6·5
Cheek	4·3	5·5
Back (lower dorsal)	0·5	3·6
Arm	4·	5·6
Forearm (back)	3·5	5·3
„ (front)	3·7	4·5
Hand (back)	2·2	4·6
„ (palm)	>0	0
Tips of fingers	3·2	5·5
Leg	0	5·
Foot (dorsum)	0	3·7
„	>0	0
Tips of toes	>0	>0

The same induction apparatus as previously was used, with two Bunsen cells. In the circuit a resistance of more than three million ohms was intercalated.

THE EYE-SYMPTOMS IN LOCOMOTOR ATAXIA.

(A CLINICAL LECTURE.)

BY T. GRAINGER STEWART, M.D., ETC.,

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GENTLEMEN,—I purpose to-day to explain and illustrate the Eye-symptoms which occur in Locomotor Ataxia. They are very common, as you will perceive when I tell you that they were present, in one form or other, in 41 out of 64 cases tabulated by Eulenburg,[1] and in 18 out of 20 cases of which I have notes. It is mainly upon a study of these 20 cases that the description I am to give you is founded.

There are four sets of symptoms:

I. Double Vision, Squinting, and Ptosis.
II. Changes in the Pupil.
III. Amblyopia and Amaurosis.
IV. Colour-Blindness.

I. *Double Vision, Squinting, and Ptosis.*

These temporary paralyses are very common affections, and manifest themselves often by double vision without apparent squint, sometimes by double vision only when the eyes are turned in particular directions, often also by distinct squint and by ptosis. The existence of the last-named symptom forbids the adoption of any hypothesis of spasm or ataxia, and shows clearly that these conditions are manifestations of the well-recognised tendency to temporary paralysis which exists throughout the course of the disease, and especially in its early stages.

[1] 'Lehrbuch d. Nervenkr., 2te Aufl. Bd. ii. s. 448.

Out of the 64 cases recorded by Eulenburg, 25 had strabismus. Of these 25, 19 had divergent strabismus, the third nerve being paralysed; 6 had convergent strabismus, the sixth nerve being affected; and 4 of the divergent cases had, in addition, paralytic ptosis. Of my 20 cases, I find that 8 had paralytic eye-symptoms, 5 having strabismus, 3 ptosis, and 4 diplopia, without manifest squint. I have not seen dilatation of the pupil occur along with the squint in any of these cases, and from this, as well as the other facts, you observe that all the branches of the third nerve are not necessarily affected together.

In illustration of these affections I may recall the case of a man, who was under observation here, suffering from locomotor ataxia in an early stage, and who exhibited no ptosis nor distinct tendency to squint, though he complained of double vision whenever his eyes were directed much to one side, or when an object was held near the eyes. Here you observe that the branch to the levator palpebræ superioris was unaffected, and in ordinary positions of the eye there was no double vision; but when the patient looked very much to one side, the third nerve of the one eye failed to produce an action equal to that of the sixth nerve of the other, and thereby double vision was produced; and again, when the eyes were converged upon a near object, the paresis manifested itself in the same way. In the case of a man, Cowan, who was for many years under my observation, and whom some of you will remember, a similar set of symptoms occasionally existed. In 1873, for example, I find it noted that he was troubled with double vision, when he looked to one side. The same man's history affords good illustration of the more obvious conditions of ptosis and squinting.

Let me mention another case, that of a man named Garrow, who was under my care in 1874. He told us that his earliest symptom was a sudden attack of giddiness, with ptosis and squinting, which occurred in 1863; that he was for thirteen weeks under the care of Mr. Walker for this malady, and only at the end of that time got completely rid of it. It recurred in the following year, and again in 1866, each time without apparent cause. Patients sometimes say that they noticed

nothing wrong, until their friends drew attention to the drooping of the eyelid or the squint.

Now these symptoms are often premonitory of the commencement of the typical symptoms of the disease. These paralyses may, however, occur after the locomotor ataxia is well advanced, and I have known them to reappear with each attack of gastric crisis, and to linger for some time after it had passed away. Von Graefe has remarked that in these cases there is a striking diminution of the tendency to the blending of images, on which binocular vision depends; that is to say, that whereas in cases of slight double vision an effort is instinctively made by movements of the eye to bring the two images into one, in this condition the attempt is scarcely made. He suggests that this points to a central origin of the affection, which view is rendered more probable by the sudden onset and temporary character of the symptom, as well as by its occurrence in connection with gastric crises.

This group of symptoms, then, you will remember is a local manifestation of the general tendency to temporary paralyses.

II. *Alterations of the Pupil.*

a. Mydriasis. This condition was observed by Eulenburg in 9 out of his 64 cases; in 3 the dilatation being double, in 4 single, and in 2 accompanied by myosis in the other eye. I have not noted this condition in any of my cases. Eulenburg states that it appears to occur mostly in the later stages of locomotor ataxia. Had it been an early symptom one might readily have explained its occurrence by one or other of two hypotheses, viz. either a paralysis of the third nerve, or an irritation of the cilio-spinal fibres. The latter is made the more likely, as Eulenburg suggests, by the circumstance that there is no defect of the power of accommodation, and that during accommodation the pupil acts, both of which facts indicate that the third nerve is unaffected. If, however, it be true that mydriasis occurs mainly in the later stages, it is difficult to understand how irritation of the cilio-spinal nerves should arise, unless in cases which have had no affection of the cilio-spinal region till a late period, and that then irritation has

preceded the paralysis. Here again is a difficulty, for were it the case that irritation always preceded paralysis, we should expect mydriasis to be a frequent early symptom of locomotor ataxia, seeing that myosis is common. Perhaps this is actually the case, and the condition is overlooked on account of the absence of concomitant symptoms.

β. Myosis has long been well known as a frequent eye-symptom in locomotor ataxia. It is referred to by Romberg, Remak, Duchenne and Trousseau, as well as by recent writers. Sometimes it is so marked as at once to attract the observer's attention. Eulenburg found it in 28 of his 64 cases, 21 showing double, 7 single myosis. Of my 20 cases, 7 had distinct myosis, and of these 4 had one pupil smaller than the other. The two pupils are seldom exactly of the same size. The degree of contraction varies greatly in different cases; it also varies from time to time in the same case, becoming, however, as a rule more marked as the disease advances, although not necessarily in a regular ratio. It may become very intense, and then again diminish. Thus I have seen it increase during gastric crises; and Charcot,[1] in his admirable lecture on the subject, states that during the attacks of lightning-pain the pupil dilates. He has also remarked that in an early stage of ataxia, it is commonly observed in inequality of the pupils, and on the side on which contraction is more marked there are sometimes evidences of vasomotor paralysis, in the reddened cheek, the congested conjunctiva, and the local elevation of temperature.

In the patient whom I now present to you, you observe extreme myosis. You notice that his ataxia is now far advanced. He often suffers from lightning-pains, and has almost constantly a broad girdle pain around the lower part of his throat. His sensibility is much diminished, patellar tendon-reflex is lost, and he has the characteristic ataxic gait, and sways about and staggers when he attempts to stand with his eyes closed. You observe that his pupils are like pin-holes, and they have been so for years. The myosis was in his case an early symptom; indeed, it was on account of dimness of

[1] 'Leçons sur les Maladies du Système Nerveux, Anomalies de l'Ataxie Locomotrice,' p. 56.

sight that he first sought medical advice. You will understand how such a condition necessarily makes the vision dim, even if there be no change in the optic nerve. If you will hold in front of your eyes a metal plate, or a card, with a very small aperture in it, such as might correspond in size to the pupil of our patient, you will find that although objects may be well enough defined, they appear dim from defective illumination.

Observe also in the left eye of the patient the result of the instillation of atropine. Instead of the complete dilatation produced in the normal eye, we have induced in our patient something less than medium dilatation. This modified effect has long been known as one of the characteristics of spinal myosis.

Myosis is due to disease of the cilio-spinal region of the cord. From that region there proceed nerve-fibres which join the sympathetic, and ultimately supply the radiating muscular fibres of the iris. When from affection of these cilio-spinal nerve filaments the radiating fibres of the iris are paralysed, there is nothing to oppose the action of the circular fibres, which by their contraction produce myosis. The pupil-symptoms of certain cases of thoracic aneurism are explained by pressure on the cilio-spinal fibres.

γ. Associated with the myosis, but sometimes occurring independently of it, is another curious feature, which was first described in 1869 by Dr. Argyll Robertson,[1] and which for convenience we are accustomed at the bedside to call "the Argyll-Robertson symptom." It consists in the absence of any contraction of pupil on exposure of the eye to light, while movement with accommodation is normally retained. This circumstance had not attracted attention until it was described by Dr. Argyll Robertson; and even now it is less known than its importance deserves. It may be well that I quote to you the description as it was originally given.

"That although the retina be quite sensitive, and the pupil contract during the act of accommodation for near objects, yet an alteration in the amount of light admitted to the eye does not influence the size of the pupil."

[1] 'Edin. Med. Journ.' Dec. 1869.

It is much more difficult to find a satisfactory explanation of this fact than of the myosis. Dr. Argyll Robertson suggested an hypothesis that the contraction of the pupil to light is not entirely due, as has been generally supposed, to contraction of the circular fibres of the iris produced through the third nerve but in part, at least, to temporary arrest of action in the radiating fibres of the iris, effected through the cilio-spinal filament, in the sympathetic. This hypothesis demanded that a function be assigned to these last-named fibres which is not known to exist in any other nerve. The considerations which induced him to hazard the hypothesis were the circumstance that, in the cases of spinal myosis which he recorded, the patients retained good vision, indicating a sensitive condition of the retina, normal conducting power in the optic nerve, and healthy state of the brain centre connected with visual impressions; further, that in them the contraction of the pupil associated with the accommodation indicated a healthy state of the third nerve and circular fibres of the iris, and yet, with an apparently complete chain of connection between the sensitive retina on the one hand, and the contractile fibres of the iris on the others light did not affect the pupil. The one pathological condition which was evidently present was paralysis of the cilio-spinal filaments in the cord, to which, therefore, he ascribed a share in the mechanism of pupil contraction under light. According to this view, one lesion seems to explain both the myosis and the Argyll-Robertson symptom.

But it was at once apparent that if this hypothesis were correct, myosis and the Argyll-Robertson phenomenon must always coexist, and I looked anxiously for evidence as to whether this was so or not. The first case in which I found them dissociated was one recorded by Wernicke, in Virchow's Archiv for 1872, vol. lvi. The patient was the subject of chronic alcoholism, and his pupils were of normal size, the right larger than the left; the former measuring 2 lines in diameter, the latter $1\frac{1}{3}$. The right reacted to light, coming down to $1\frac{1}{4}$ lines; the left did not react to light, but both acted with accommodation. Now it is true that in this case the pupil which manifested the phenomenon was smaller than the other, but it was not by any means very small. Since that time I

have met with at least one well-marked instance in my own practice in which the pupils were of normal size and were equal, but the phenomenon was distinctly present. It thus appears that this suggestion does not afford the desired explanation.

Wernicke and Hempel adopt the view that the nervous connection between the optic and oculomotor nerves is interrupted,[1] and consequently the reflex irritation of the oculomotor can no longer occur, but the centre of the third is not destroyed. It appears, on the whole, most probable that the movements of the iris on exposure to light, and those for accommodation, are controlled by different nerve centres, and that in this disease the one centre and not the other is affected. Further anatomical and physiological investigations of course may be expected to clear up this question; and I have some hope that certain preparations in my possession may contribute to this end.

As to the frequency of the occurrence of this phenomenon, I know of no statistical statement, but of my 20 cases it was present in 8. In 7 of these it was associated with myosis; in 1 not, the pupils being in that case of normal size. It is not present in every case of myosis. It seems not improbable that its absence in such cases may depend upon its being a later symptom in the order of development.

III. *Amblyopia and Amaurosis.*

Dimness of sight is also a very common symptom, but it rarely goes on to complete blindness. Besides the degree of blindness arising from the cutting off of rays by the contracting iris already referred to, we find in some cases another form of dimness, temporary and apparently functional, and corresponding to the temporary paralyses so often seen in the earlier stages of the disease. I found, for example, that a man who was under my care in 1872 had suffered from an attack of dimness of vision eight years before, and another eighteen months before I saw him, both attacks being temporary and unexplained. In 4 out of my 20 cases similar facts are noted.

[1] Graefe's 'Archiv,' 1876, vol. xxi., Part I., p. 22.

It seems safe to conclude that such temporary seizures depend, as do the temporary muscular paralyses, on some undetermined condition.

But far more important is the permanent and slowly advancing amblyopia. Eulenburg found it in 31 of his 64 cases, and it was present in 14 of my 20 cases. In 7 of the 14 it appeared to be one of the earliest symptoms; in 5 of the cases there was distinct atrophy of the optic disc.

This condition is well illustrated in the patient before you. The sight of his right eye has been gradually diminishing, and is now quite lost, so that he cannot distinguish light from darkness. The sight of the left eye is also impaired. The ophthalmoscope reveals in both of the discs marked atrophic change, most advanced, of course, in the right. The features of this condition are admirably described by Charcot and sketched by Erb. I quote the description given by the latter (Ziemssen's Cyclopedia, Eng. Trans. vol. xiii. p. 579).

"But the atrophy of the optic nerve, which is unfortunately very frequent in tabes, is of much more importance than the disturbances of the visual apparatus thus far considered, and is much harder for the patient to endure. It belongs to the saddest complications of this already melancholy disease; the helplessness caused by the ataxy is aggravated beyond all measure by the blindness added thereto, and the sources from which the patients can obtain comfort and oblivion amid their suffering are materially diminished.

"The trouble begins with slowly or rapidly advancing diminution in the sharpness of vision, which soon increases to amblyopia, and finally to amaurosis. The field of vision, which is at first slightly veiled and cloudy, becomes increasingly narrowed, generally from without inward, sometimes from without and above, sometimes more from below. Examination shows that this narrowing of the field of vision does not take place in a uniform manner, but with entering angles. Finally, the blind area involves all but a spot towards the inside, which allows the patient but a limited and insufficient amount of vision. . . ."

"Ophthalmoscopic examination reveals the signs of so-called white atrophy of the optic nerve. At first there is a slight

grayish discoloration of the papilla, which gradually grows paler, and finally appears quite white and sharply outlined. The arteries at the same time show a progressive narrowing, but otherwise the retina remains quite unaltered. These changes are easily to be distinguished from the ophthalmoscopic picture of neuritis optica and congested papilla, and, according to French authors, they are so characteristic that the existence on the approach of tabes can be recognised from them alone.

"Atrophy of the optic nerve in tabes is always—with quite rare exceptions—a decidedly progressive malady, and leads, without interruption, to complete amaurosis. In different cases, it is true, this occurs with very variable rapidity; sometimes it requires only weeks, sometimes months or even years, for its accomplishment. Occasionally the malady comes to a standstill, even after it has lasted for a comparatively long time. Sometimes the trouble is limited for a considerable time to one eye, but it is far more common for both to be attacked and to become blind simultaneously, or within a short time. Modifications between these two extremes are of course manifold."

Such dimness of sight and white atrophy of the nerve is by no means confined to locomotor ataxia, for it sometimes occurs without any central lesion, sometimes with cerebral disease, and sometimes with other spinal affections.

In illustration of this, I show you a patient suffering from antero-lateral sclerosis, who has for upwards of a year had well-marked amblyopia, with white atrophy of the optic nerve. In all these cases the anatomical change is essentially the same with that which exists in the spinal cord.

IV. *Colour-Blindness.*

This is another symptom, fully recognised as occurring in locomotor ataxia. You have seen recently, in Ward I., an illustration of this condition, and I shall presently show you a well-marked example. It is not so common as the other eye-symptoms. Of my 20 cases it was noted only in 3. One of them is the patient now before you. His colour-blindness has

existed for a long time, and it appears to vary from time to time. Thus, in February 1877 he could recognise green, white, pink, and black; but scarlet he called yellow. A year later he called green, dark-brown; red, black; yellow, he called light, but not white; and in February 1879 he could discern blue, yellow, and red. I now show him a series of sheets of coloured paper; the blue, he says, is white; the red is like blot-sheet, the green is drab, the pink is white, the yellow is yellow, the orange-brown is yellow, and the white is recognised as white. He is thus considerably worse than he was four months ago.

It is stated by Erb that colour-blindness can usually be demonstrated before any limitation in the field of vision, and he states that generally the perception of green is lost first, then that of red, finally that of yellow and blue.

Charcot states that the achromatopsia is characterised by loss of perception of the secondary colours, and by loss of perception of red and green; perception of yellow and blue persisting in a high degree, and for a long time. The facts I have shown you in our patient do not strictly correspond with these statements.

It is clear that colour-blindness does not necessarily accompany the amblyopia of locomotor ataxia, and we have not at present sufficient evidence to determine the question, whether it is more frequently associated with this form of amblyopia than with others. I have also no knowledge of any case in which colour-blindness alone was present, and am therefore inclined to attribute it to some peculiar distribution of the atrophic condition in the optic nerve.

I have not deemed it necessary to draw your attention specially to other eye-symptoms which have been described, because they are certainly of very rare occurrence, and, so far as I can judge, of but little importance.

REMARKS ON BULBAR PARALYSIS, WITH SPECIAL REFERENCE TO ARTIFICIAL FEEDING.

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THE consideration of this subject is to my mind of such great importance, that I have no hesitation in bringing it before the readers of 'BRAIN,' because I do not find that it is insisted upon in those papers and articles that have come under my notice relative to the treatment of this disease. For instance, in Erb's article on "Progressive Bulbar Paralysis" in Ziemssen's 'Encyclopedia of the Practice of Medicine,' we find the following:

"When deglutition becomes impossible, we must feed the patient through a flexible tube; but with many patients this is rendered impossible by violent fits of coughing, vomiting and suffocation. In this case nothing is left but the administration of nutritive enemata, such as solution of pancreas, milk and eggs, bouillon, wine, &c."

For many years past I have, in speaking or writing on the treatment of this affection, been particularly careful to draw attention to the necessity that arises for artificial feeding *before deglutition becomes impossible*, and as soon as it is in any way interfered with.

I do not wish it to be inferred from this that no nourishment should be allowed to pass into the stomach except through the tube, but I would have the bulk of nutritive material introduced in this way. The call for nourishment and the rapacity of the individual is astonishing; and as the difficulty in

swallowing increases, so does the desire for food, and in direct ratio.

It must be understood that the nature of this communication is practical even to commonplace, so that its real meaning shall be at once grasped even by those who do not care to study psycho-physiological problems.

The connection between the medulla oblongata and the bulb with the cord below and the central nervous system above, is recognised even by advanced scientific investigators of the present day to be of the most complicated nature; and when we consider the immediate connection of the medulla oblongata with the entangled web of fibres in the pons, we are at no loss for evidence to show how difficult it not unfrequently is to make a correct diagnosis in reference to pontine and bulbar lesions. We can thus see how readily many observers are and have been led into error, and that many palsies, including those of the muscles of deglutition, which have been called bulbar paralyses, are really not so, but take their origin from some disease either below or above the medulla oblongata. I know of no published record at the present time where this differential diagnosis has been carried out with anything like completeness and precision, although many valuable articles written upon this subject are to be found.

In all probability this is due to our want of knowledge of the connection between the histological elements and the course of the nerve fibres in the medulla oblongata. Erb says:

"Notwithstanding all the investigations that have been made, and the incalculable trouble expended upon this subject, we are scarcely acquainted with the barest outlines. We know just as little about the fate of most of the fasciculi which enter the medulla from below and from above as we do of the significance of numerous striking structures in the medulla. Their connection with one another and with the gray nuclei, the mode of interruption and where it occurs, their transposition and their termination, all remain a mystery."

But the study of the bare pathology of medullary disease does not give us so much trouble as tracing the histological connection of its bundles of fibres, and provided we find

clinical signs which are indicative of lesions of those nerve centres which exist in the floor of the fourth ventricle in immediate connection with the bulb, we are satisfied, as far as our clinical work is concerned.

But perhaps of the two conditions we are as physicians more immediately interested in the vascular supply of the medulla oblongata as productive of pathological changes.

M. Duret[1] has shown that the medulla receives its blood supply from the vertebral and basilar arteries, the median branchlets which pierce into the raphe between the pyramids, and are given off by the art. spinalis for the bulbus and by the basilaris for the pons. But besides these, small vessels pierce the medulla along with the nerve roots, and run to the corresponding nuclei, where they take part in the formation of the capillaries. The olivary bodies and pyramids are supplied by small branches from the vertebral and anterior spinal arteries. The restiform bodies receive vessels from the art. cerebelli infer. post., whilst the post. spinal arteries send branches to the pedunc. cerebelli and floor of the fourth ventricle. I have always maintained—and my views are confirmed by experience—that whenever we have direct disease of the bulbar nerves and nervous centres, it is invariably due either to tumour, to change in the coats of the vessels, or to the production of thrombi within the vessels themselves.

I have no hesitation in saying that bulbar paralysis, pure and simple, is one of the rarest forms of disease of the nervous system with which neurologists are acquainted, and that it is more frequently than otherwise associated with progressive disease of the optic thalami, crura and pons from above, or with ascending disease of the spinal cord.

The troubles of speech are not necessarily associated with bulbar disease, but, as M. Voisin has pointed out with good show of reason, where the speech is of a jabbering, stammering kind, it is usually the result of disease of the medulla oblongata.

Many cases which have been designated labio-glosso-laryngeal and pharyngeal paralysis, and stated to be due to

[1] Duret, sur la distribution des artères nourricières du bulbe rachidien, *Arch. de Physiol. et Pathol.*, 1873, p. 97.

disease of the bulbo-rachidian nerves, have not in reality been so. Notably some of those reported by M. Trousseau and Duchenne, and by others as retrogressive disease of the bulb. They have been, in fact, instances with which we are now quite familiar, of primary lesions existing in a given motor convolutional area, or in the basic cerebral ganglia extending progressively towards the bulb, and disturbing and eventually annihilating its function. Yet, for the sake of demonstration, such cases as these come especially under our observation when we have to consider their treatment by artificial feeding. When we note with care the attendant symptoms and signs of bulbar paralysis, such as the affections of speech, mastication, deglutition, expression, respiration, circulation, and the continuous flow of saliva from the mouth, our diagnosis is practically simple. It is foreign to the object of this short paper to differentiate in any way between these signs, but we must glance at the disorders of nutrition, not only with the parts immediately in relation with the bulbar nervous supply, but of the body generally. Atrophy of the muscles supplied by the bulbar nerves is of regular and constant occurrence in this disease; and in almost every case, with some few exceptions, the atrophy is regular, progressive, and uniform; but perhaps it is more marked in the tongue, which looks limp, becomes furrowed, small, flattened, shrunken, and is in a constant state of vibratory motion due to fibrillary contractions. The patient's want of voluntary power over this member is extremely pathognomonic, and the first indication evidenced may or may not be due to defective articulation. There can be little doubt that it is usually so. Yet, as pathologists, we are perfectly aware that considerable disease can exist in the large ganglion cells of the motor nerve nuclei in the floor of the fourth ventricle, prior to any observable changes going on in the muscles supplied by these nerves. Not that the muscles themselves are escaping the inevitable and ultimate changes which must follow, but rather that these changes are not in the first instance, or even for some time, so impaired as to have their normal function objectively interfered with.

And here we come to a part of the subject to which I have always ventured to draw particular attention. In making an

early and correct diagnosis, if we hope that our treatment shall be attended with success, it is necessary to watch carefully the signs of fatigue of the muscles of the lips, tongue, and pharynx: in the first place, from a normal standard in relation to their individual functions; secondly, from a normal standard in relation to their correlative and co-ordinating functions; thirdly, in reference to these in combination with enforced or extraordinary volitional, automatic, and compound co-ordinating functions. By this means we shall be able to ascertain the almost immediate changes, or general nutritive disturbances which take place from those changes, which are undoubtedly of primary origin in the motor nerve nuclei—at all events, in the form of paralysis now under consideration.

So thoroughly conflicting are the statements as to faradic and galvanic irritability of muscle and nerve in bulbar paralysis, that one feels inclined to discard them altogether as sources of error and fallacy; but my colleague, M. de Watteville, certainly seems to have studied the question with considerable care, and to have elucidated some results which are capable of confirmation, if not of practical application in diagnosis. He says ('Introduction to Medical Electricity,' p. 99), "That electro-diagnosis, especially in the earlier stages of bulbar paralysis, points to its connection with other diseases of the grey matter, such as progressive muscular atrophy, and amyotrophic lateral sclerosis. Careful investigation shows in these diseases a diminished electro-muscular reaction to both faradic and galvanic current, with a tendency to a preponderance of the positive over the negative pole. Electro-nervous irritability is not necessarily affected; the disease then differs from poliomyelitis anterior by not giving the full reaction of degeneration, but only the incomplete form of it so well studied by Prof. Erb, of Heidelberg."

Erb writes, Ziemssen's 'Encyclopedia of Medicine,' vol. 13: "Encouraged by my observations on progressive muscular atrophy, and led on by the conviction that the form of degenerative atrophy with which we have to deal in bulbar paralysis would be physiologically expressed by the 'reaction of degeneration,' I expected certainly to find this reaction in the disease, and my expectation was fully gratified by the

first pronounced case which came under my observation, and which I examined with great care. I found in fact the most marked reaction of degeneration in irritation of the muscles both on the chin and in the lips, and even in the tongue. At the same time, however, the electric irritability of their nerves was normal, or but slightly diminished, so that we have exactly the same form of degenerative reaction which I have described as characteristic of the so-called 'middle form' of certain peripheral paralyses, and I have no doubt we shall find the same thing if we look carefully for it in all cases where atrophy of the muscles has reached a certain period."

From personal experience, I have relied little upon either electrical or galvanic phenomena, for it is quite clear that muscles in their natural state vary considerably in their contractile power, either through the agency of the faradic or continuous currents. I would, in making diagnosis, depend chiefly upon the clinical evidence which has been brought to light by enforced fatigue, or strained co-ordinating effort. The act of mastication, though purely voluntary in a healthy state of the centres of the bulbar nerves, becomes less so in proportion as these centres are directly or indirectly involved, and the important part played by the hypoglossal nerve through the muscles which regulate the movements of the tongue becomes so strikingly manifest during even the incipient stage of bulbar disease, that we are compelled here to advert to it, and to the process of deglutition. Complex and voluntary as these movements are, they differ little from other complex movements, in being to a great extent automatic or reflex, and volitional power exercised over them is rather the exception than the rule. Something gets between the teeth, or between the teeth and the cheek, which cannot be easily removed, and the branches of the fifth nerve convey the special muscular sense and contact sensation to the brain, which send forth motor volitional impulses from this centre, to effect by an increased volitional act what the mere reflex automatic act is unable to effect. Now in the act of deglutition we readily admit that we have, as is well known to the student of physiology, processes which are far more intricate and delicate than the mere mechanism of mastication. Guided

by our text-books rather for mathematical exposition than from pure physiological reasoning, we find that the first act of deglutition is said to be voluntary; the raising of the soft palate and the approximation of the post pillars of the fauces are intimately associated with the tongue in passing the bolus of food into the upper part of the bag of the pharynx; but after the food has passed the anterior pillars of the fauces, then the act of deglutition is essentially reflex, and the will is said to have no power over the constrictors either to originate or modify their action. If the bulbar centres are diseased, and if we admit that the reflex centre exists in the medulla oblongata, it must be evident that the act of deglutition becomes not only voluntarily but automatically and reflexly most seriously impaired; and it is not, let it be understood, the first act of deglutition which gives the patient most serious and agonizing distress; but it is in the second and third stages through hyper-reflex and abnormal excitations which are so distressing. And here I am forced to support a doctrine which at times must occur to the practical physician, that normal physiological acts have not unfrequently rather an indirect than a direct bearing upon anomalous diseased states. In some cases of bulbar paralysis we know that reflex actions are strikingly diminished. We can touch, irritate, and apply a strong continuous current to the pharynx, soft palate, and even the larynx, without exciting them. Yet the patients have little difficulty in localising the sense of touch; but such a want of reflex sensibility does not usually exist in that early stage of the disease where we consider artificial feeding to be so supremely essential. If in bulbar paralysis we had to combat the patient's unwillingness and determination not to take food, as we so frequently have to do with insane patients, my paper would fall short of its object; but what I hold is this, that in the form of paralysis which we have under our notice, two things are absolutely essential to its rational treatment, namely, a highly nutritious and assimilable diet to be given at frequent intervals, and as far as possible to keep the muscles of expression, articulation, and deglutition, in a state of psychological no less than physiological rest. This must be at once apparent to the physician who has carefully

noted the onset and progress of these cases. The suffocative cry of despair uttered by the unfortunate creatures when they feel themselves powerless either to eject or swallow the food lying passively on the isthmus of the fauces, or between the gums and the cheeks, tells its own tale. In one case which was under my care, a young fellow, exhibited at the Medical Society of London, improved considerably under treatment. His appetite was simply voracious: he was fed regularly through the nose by means of a piece of india-rubber tubing, which he usually passed himself well into the œsophagus; but, independently of this, he stole food whenever he could, and the only way he got it into the pharynx was to cram his mouth to its utmost limit, and then push the food with his finger down the throat.

Upon his visit to the Medical Society[1] the different processes of nasal feeding were put to the test. Dr. Tuke's highly finished and somewhat catheter-like instrument could not be introduced, but an ordinary piece of india-rubber tubing of less than $\frac{1}{2}$-inch diameter passed without any trouble.

A few remarks about artificial feeding in general will conclude my paper, with a word or two concerning nasal feeding.

Feeding the insane through the nostrils was first prominently brought before the profession, and its advantages ably maintained by Dr. Moxey, in the columns of the 'Lancet, 1869–73, although Dr. Clouston amongst others wrote rather biassedly in favour of the stomach-pump and gag. I think there can be little doubt that the no less scientific, though perhaps unnatural, mode of procedure, of making the passages of the nose a means of direct communication with the involuntary muscles of the œsophagus is now generally admitted and usually adopted. For my part and in my experience, the latter has exceptional advantages of which I have always availed myself, no less in cases where forcible feeding has been required than in those to which this paper especially refers. To prevent reflex irritability, the tube should pass beyond the muscles of the pharynx, then the food enters the stomach in a continuous stream, no matter what efforts are

[1] 'Med. Soc. Proceedings,' vol. i.

made on the part of the patient to prevent it. I cannot but apprehend considerable inconvenience may occasionally arise from merely pouring the fluid through the nostril, unaccompanied by tubing. Concerning the difficulty which sometimes occurs in passing a tube through the nostrils into the œsophagus, I am quite aware that it depends no less upon the skill of the operator than upon the nature and size of the tubing used. The introduction of a bougie, or catheter, through the male urethra, is by comparison a teaching example. We have all found by practice that a No. 10 elastic catheter will glide into the bladder with perfect ease and total absence of pain to the patient, when a No. 4, if passed at all, is accompanied by excruciating pain and discomfort. It is not less so in the passage of the naso-œsophageal tube, and I have more than once been compelled to desist from attempting to pass a highly-wrought beautifully tapering elastic œsophageal tube, when a piece of common india-rubber tubing has been subsequently passed with facility. In forcible feeding, we have on the introduction of the tube not unfrequently to overcome volitional spasm of the pharyngeal muscles, by preventing the access of air to the patient's lungs, when in a few seconds the automatic compulsory respiratory effort soon necessitates a deep inspiration, and the difficulty is at once removed by the immediate passage of the tube. In spasmodic strictures of the urethra, prolonged gentle firm pressure causes the primary spasm to yield, and the obstacle is not only overcome but the temporary muscular inertia produced by overstrain renders the passage of the catheter doubly easy. We find at times, when the operation of nasal feeding is conducted imperfectly and hurriedly, that reflex irritation gives rise to a sense of choking and perhaps sputtering of fluid from the mouth and nose, but this is simply due to the absence of careful manipulation. For some years past my œsophageal tube and funnel for nasal feeding has consisted of an ordinary india-rubber inflation pessary (a hole being cut in the bulb which forms the funnel to admit of the pouring in of the fluid). The patients usually introduce the tubes themselves, and the nurse pours in the fluid nourishment. A man, with dysphagia and an incomplete bulbar paralysis, was artificially fed on this system,

and some few short notes of his case may be of interest. He was under my care for a month, and increased eight pounds in weight, and improved equally in other respects. W. C., aged 63, was admitted into the Central London Sick Asylum at Highgate, on March 22, 1877. It was not an easy matter to get a clear history from him, as his want of intelligence and reasoning powers were more or less at fault. Until five years ago his health was excellent. It appears that in the evening after his day's work, when he was sitting by the fire smoking his pipe, he became giddy and fell from the chair, but without losing consciousness. From this time he never recovered his usual health. There has been more or less weakness in the limbs, but particularly those of the *left* side. His speech and power of swallowing were unaffected by this seizure. In the month of October, four years ago, he had a similar seizure; but it was not until Feb. 1, 1877, that he quite suddenly lost both the power to swallow easily and speaking distinctly. He describes the last attack in this way. He was going to drink some beer, and found that he was unable to swallow one drop of it. On the following morning he tried to work but could not, because his arms felt so weak; he could not swallow, neither could he speak so as to be understood. When I first saw him he was extremely emotional; there was an equal falling of the under lip, and a drawing downwards of the angles of the mouth, with an overflow of saliva, which he was constantly rubbing away with the back of his hand. I was led then to conclude that he was suffering from an affection of the bulb, either direct or indirect. This was soon proved, as we saw by examining the cranial nerves in the following order.

Nerves:—

1*st*. Sense of smell fairly good; equal on both sides, but not acute.

2*nd*. Sight has been failing since the first attack. Says that he has often fancied two sixpences were three, and that halfpence were pence. The ophthalmoscope gave evidence of advancing atrophy of the optic discs, and partial obliteration of vessels.

3*rd*. Normal.

4*th*. Slight palsy of.

5*th*. Pterygoids involved. Can depress the lower jaw. No loss of sensation of mucous surface of either cheeks or gums.

7*th*. Can whistle, but not spit. No flapping of buccinators when he tries to blow, but the column of air sent out is very feeble. When he laughs, the angles of the mouth are well drawn up. Hearing normal.

8*th*. Says he is a little short of wind. Marked dysphagia. No absence of sensation.

9*th*. The glossoplegia is incomplete. He can just protrude the tongue over the lower teeth, and raise the tip to the palate. The mucous membrane covering the tongue has not undergone the usual atrophic change one meets with in true bulbar disease. When he gets food into the mouth, he is unable to use the tongue to remove it; but after it has passed through the isthmus of the fauces, it is swallowed without much difficulty.

It must be particularly noted that there is marked absence of voluntary co-ordinating power between the lips and the tongue, and when a voluntary act of the one is attempted, an involuntary act of the other is frequently the result. In reference to speech, there is greater defect sometimes than others. It is usually of the mumbling hesitating character. All the vowels and labials are well pronounced, but the letter C is pronounced "Pthsee," and G, "Dgee." The letter R cannot be articulated. The vocal cords act well, but he cannot talk in a loud voice, or holloa. There is marked inability of the muscles to raise the larynx. The muscular power of both arms is weak, but co-ordinate, and in like manner the legs are affected. He can raise 35lb. with each hand, and 14lb. with each leg when reclining. There is and always has been perfect control over the sphincters. The urine is of low gravity (1010), and free from albumen.

Now it is clear upon casting one's eye over these notes that the signs of paralysis of the muscles supplied by motor nerve fibres, originating in the floor of the fourth ventricle, were in excess of paralysis of those muscles supplied by other motor cerebral centres. Yet we find no absence of sensation which is so usually an accompaniment of bulbar paralysis.

The faradic test was not applied. The precise seats or area of pathological change in this case were undoubtedly a little obscure, and I anticipated they were general as well as circumscribed; but there could be no doubt concerning the very marked improvement which resulted from artificial feeding. I maintain it would have been impossible to have produced so satisfactory a result in so short a time by any other means, and I have brought it forward as one of those cases which proves practically how essentially necessary it is to aid nutrition to its utmost extent artificially, rather than allow our patients to die from inanition and never-ceasing mental disquietude, if not absolute dementia.

ON AFFECTIONS OF SPEECH FROM DISEASE OF THE BRAIN.

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In the first* instalment of this article ('BRAIN,' Vol. I. Part III., p. 304) it was pointed out that there are necessarily numerous degrees and kinds of affection of language, since "different amounts of nervous arrangements in different positions are destroyed with different rapidity in different persons." Moreover cases are vastly different in their different stages; a patient may be quite speechless for a few days, and afterwards improve so as to have at length only slight defect of speech; and of course there are numerous cases of complete recovery. It is necessary to make some division of cases; we roughly made three groups (see p. 314, Vol. I.). We were careful to declare that this division was an arbitrary one, that it was not a scien-

* I should like to remark that one very general conclusion to which the several facts so far stated, and facts afterwards to be stated, point, was in principle long ago formulated by M. Baillarger. So far back as 1866, 'Med. Times and Gazette,' June 23, I made the following quotations from his writings, which I now reproduce:—

"L'analyse des phénomènes conduit à reconnaître, dans certains cas de ce genre, que l'incitation verbale involontaire persiste, mais que l'incitation volontaire est abolie. Quant à la perversion de la faculté du langage caractérisée par la prononciation de mots incohérents, la lésion consiste encore dans la substitution de la parole automatique à l'incitation verbale volontaire."

I ought to have reproduced this quotation in the first instalment of this article, as evidently I am following pretty closely the principle this distinguished Frenchman has laid down. For the satisfaction of curious persons, I may say that I give it now spontaneously, no one having drawn my attention to the omission. I fear M. Baillarger's acute remarks have attracted little attention, and I say with regret that I had forgotten them. I do not remember from what book I took the quotation.

tific distinction. The divisions usually made are arbitrary too, although the nomenclature being in highly technical psychological and clinical terms, they may appear to the unwary as being real, almost natural, distinctions. As was then insisted on, we must in an empirical inquiry take type-cases; we follow the plan which is tacitly, if not avowedly, adopted in every work on the "Practice of Medicine" with regard to all diseases.

We took for first consideration the simplest group—cases of Loss of Speech (No. 2), p. 314; Cases of Defect of Speech (No. 1), and that deeper involvement of language in which emotional manifestations (No. 3) scarcely remain, are more difficult, and will be considered later. We take the simple case for investigation first, just as, were we writing on hemiplegia, we should take first the simplest case of that paralysis, not the more difficult case, in which there are deep loss of consciousness and lateral deviation of the eyes and head, as well as paralysis of the face, tongue, arm and leg. A patient who loses speech may regain it; it is convenient to consider cases of permanent speechlessness.

On pp. 316 *et seq.* we made some brief general statements as to the speechless patient's condition; we especially insisted on the necessity of recognising a positive as well as a negative element. I feel convinced that unless in all degrees of affection of Language we recognise that the symptomatic condition is duplex, we shall not trace relations betwixt them, and shall be misled into supposing that cases are different in kind when there are only differences of degree. We have remarked on this, p. 316. Further, we shall not be able to trace analogies betwixt these examples of Dissolution beginning in the lower cerebral centres, and cases of Dissolution beginning in the highest centres, that is to say, cases of Insanity where the condition is manifestly duplex. The most important thing showing the duality of the speechless man's condition is given very generally by saying that Speechlessness does not imply Wordlessness. We stated that there is not evidence that the process of perception is damaged in itself; we say "in itself," admitting that perception may suffer from lack of co-operation of speech-use of words.

We have now to consider more particularly the condition of the patient we call speechless. In most cases there remains some utterance. But to utter words is not necessarily to speak. To speak is to propositionise. We admit, however, that in some cases which we call loss of speech there is a recurring utterance, viz. "yes" or "no," which is of propositional value; and that occasionally there occur utterances which are of propositional value, and are made up of several words. But in these cases the recurring utterances which have propositional value are so very general in their application, and the occasional utterances which have propositional value are so rare and usually also of so very general application, that it is almost pedantic to say the patients are not speechless. Nevertheless there are exceptions to our statement that there is *loss* of speech in the type-case (No. 2), and full consideration will be given to them.

We divide the utterances into two classes, Recurring and Occasional.

(1.) *Recurring Utterances.*

Soon after the attack, there may be no sort of utterance. But almost always one comes in a few days or weeks. I used to call them "Stock Utterances." They are always utterable; and they alone remain, with the exception of the rare occasional utterances. We make four divisions of Recurring Utterances.

(1) It is sometimes jargon. In one case it was "Yabby," in another, "Watty." Sometimes there is a succession of different jargon; in one case, "Me, me committimy, pittymy, lor, deah." The patient utters his jargon any time. If he "says" anything, it is always "Yabby," or whatever his jargon may be; in reality he *says* nothing with these utterances; they have no propositional value whatever.

(2) Sometimes the utterance is, what to a healthy person is, a word, as "man," "one," "awful," &c. Such a word is, for use, no better than jargon in the mouth of the speechless patient; it is not a word to him; "man," as a recurring utterance, is not a symbol for a human being. The so-called word comes out, just as "yabby" does, and means no more,

means nothing. A single word might have, in a healthy person, propositional value. For example, were a person asked how many oranges he would buy, the reply "one" would be a proposition. (See p. 312.) But the speechless man's recurring "one" comes out whenever anything comes out, and applies to nothing at all.

Here, having given some examples, I may make one general remark about all kinds of Recurring Utterances. The rule is that the particular recurring utterance each person has at first never changes. Sometimes, however, but exceedingly rarely, it changes. A patient of mine for some months, when under my care in the London Hospital, could only utter the word "Dick." Later it changed to "Jimmy," with the variations of "Jim" and "Jigger." Another general remark is that although these rags and tatters of what was once the patient's speech are of no use as speech, they serve as parts of emotional manifestations; it is rather, we should say, the tones in which they are uttered; it would be most correct to say the patient "sings" his recurring utterance—variations of tone with healthy speech being rudimentary singing (Spencer).

In this service of these, as also in that of the other recurring utterances, we have evidence that Emotional Language is not affected.[1]

The way in which speechless recurring utterances serve patients is exemplified in some cases noted in the Hospital Reports of the *Lancet*, February 17, 1866, and July 20, 1867. Several other things of importance for other departments of our subject are given in the following extracts.

"The patient we saw could only utter the word 'Dick,' and this word he uttered whenever we asked him a question. We were told that when the man was vexed by the other patients in his ward he would swear. He generally used the common explosive sound, so much in favour with English swearers. He could not, however, say the word when required to do so, even whilst it was well kept before his mind by frequent repetition. He seemed to make efforts to say it, but the word

[1] I would not affirm that the finest emotional manifestations may not be lost in cases of loss of speech; I do not know that they are. It would be very remarkable if they were not.

'Dick' always came out instead. The oath was only uttered under the influence of emotion, and could never be repeated at will."—The *Lancet*, Feb. 17, 1866.

The following is a further note of the same case from the *Mirror* of the *Lancet*, July 20, 1867.

"When the poor fellow left the London Hospital he was able to utter the word 'Dick' only, except that he swore when vexed. He is now in a workhouse, where, thanks to the permission of Dr. E. H. Moore, Dr. Hughlings-Jackson saw him a few weeks ago. Strange to say, the patient's stock phrase is now 'Jimmy;' he never says 'Dick.' Although it is two years since the patient left the hospital, as soon as he saw the doctor he raised himself eagerly from his chair, offered his left hand—his right is still paralysed—and cried out very vivaciously, 'Jimmy, Jimmy,' &c., evidently pleased to see some one whom he knew. The ward superintendent says the patient sometimes sings; that the word he then uses is 'jigger.' He is usually quiet; but when vexed he swears, or rather utters a very nasty word, the last syllable of which rhymes to the last syllable of jigger. He cannot say this word when he tries, but, when trying, says 'Jim' instead. 'Jimmy' seems to be the word he uses as an ejaculation to show states of feeling, and 'Jim' when he is trying to convey information. When asked to show how many children he had, he extended his left five digits twice, and at each extension he uttered jerkingly the word 'Jim.' At a second visit, the man replied to the same question in the same way; but there are no means of knowing whether his reply is a correct one or not. He does not tell the number of days in the week by this plan. He sang when asked; and although the performance was of the very poorest kind, there was cadence with variation of tone. In one of these efforts he used as a vehicle of sound the word 'Jim,' in another, 'jigger.' The ward superintendent remarked that the man's friends had not visited him since Christmas. Here the patient clenched his fist, tightened his lips, face, and neck, holding his breath, and turning red the while, as if making an effort. After a moment or two he sighed deeply and relaxed, shook his head, and looked as if he had given up an attempt to do something. The attendant believed the

patient was 'trying to talk,' and said he often saw him put himself in that way. As Dr. Hughlings-Jackson was leaving the room, the patient left by another door, but in passing through the doorway he stopped, and turned his head as if he had suddenly remembered something, looked towards the doctor, and said pleasantly, 'Jim, Jim.' It was supposed that this meant good-bye."

I may here mention that I did not get to know if the patient had sons of the name of Richard and James (*vide infra*).

The following is from the same *Mirror* of the *Lancet* as the last quotation. It shows a certain *use* of the jargon "ow," during the expression of number, as well as its use as the proposition "yes."

"In another workhouse Dr. Hughlings-Jackson saw, with Dr. Edward Richardson and with his assistant, Mr. Widdas, a woman twenty-five years of age, who is only able to utter the phrase 'Oh! my God!' and the noise 'ow'—probably a corruption of oh! When the doctors went up to her bed and spoke to her, she cried out 'Oh! my God!' When next spoken to, she said 'Oh!' and then put her hand over her mouth. She uttered the phrase several times in the interview; but she 'spoke' with the syllable 'ow,' expressing assent or dissent by the tone she gave to it, and by her manner. She was asked how long it was after her confinement before the loss of speech came on. She held out her five left fingers, and said 'ow,' and then separating one finger from the rest of those of the paralysed right hand, again said 'ow.' The doctors said interrogatively, 'Six?' She nodded, and said 'ow.' They then asked whether weeks, months, or years, really themselves knowing the right time. By variation of tone of 'ow,' by nodding and shaking the head, she expressed assent or dissent when the right or wrong period was named. She laughed heartily when something jocose was said, crying out 'Oh! my God!' When the death of her baby was mentioned, her eyes filled with tears. The nurse says the woman was once in her ward before, and then the words uttered were 'Oh! my goodness will!'"

(3) The Recurring Utterance is sometimes a phrase. In one case "Come on," or sometimes that patient uttered "Come

on to me." In another case, just mentioned, it was, "Oh! my God!" In another case, mentioned to me by Dr. Langdon Down, "Yes, but you know."

In some cases, as in the one first mentioned, the patient may utter "yes" or "no," or both, in addition to his recurring phrase—see (4). He has then two sets of recurring utterances.

These phrases, which have propositional structure, have in the mouths of speechless patients no propositional function. They are not speech, being never used as speech; they are for use only compound jargon; they or their tones are at the best of interjectional value only. The man who uttered "Come on to me," uttered it on every occasion when he made a rejoinder to anything said to him.

(4) A common thing is that the patient retains as his sole utterance "yes" or "no," or both these words. Sometimes there is in addition some utterance of one of the other divisions. This must be carefully borne in mind. We shall consider the utterances "yes" and "no" at length. It is the most important part of the whole inquiry. The consideration of these and of some other fundamentally like phenomena will help us out of the empirical stage of divisions into the scientific one of distinctions.

To speak is, as has been said, to propositionise; many verbal utterances by the healthy are not speech. Now the words "yes" and "no" are propositions; indeed to call them "words" is not to acknowledge their proper rank; "proposition-words" might be a more correct expression. But they are not always propositions—are not always used for assent and dissent; and thus the term "word" is convenient, if not strictly accurate.

It does not matter what the philological history of the words may be; at any rate "yes" and "no" stand for propositions. They are propositions in effect; we can *say* with them. Nor must we limit ourselves to the very syllables "yes" and "no." One of my patients had the utterance "Eh," which was "yes" for him, and possibly was a corruption of his healthy "yes." Were we now dealing with the less special part of intellectual language, pantomime, we should admit nodding the head for assent and shaking the head for dissent to be pantomimic propositions. With the other recurring utterances, (1), (2), and

(3), the patient says nothing; they are a mere series of syllables; the so-called words and phrases (2 and 3) being intellectually dead. Is not there then in the utterance of "yes" and "no" a real exception to the statement that our patient is speechless?

An utterance is or is not a proposition according as it is used.

(*a*) The speechless patient may utter "yes," or "no," or both, in different tones, merely according as he is thus or thus excited. It is then not a proposition, but an interjection, a mere vehicle for variations of voice, expressive of feeling. (*b*) He may have this service of the words and be able also to reply with them; the latter is a propositional use of them. (*c*) He may (in addition to (*a*) and (*b*)) be able to say the words when told to say them.

(*a*) A speechless patient may utter "yes" and "no" without any sort of application. He may utter "yes" when he means "no," and "no" when he means "yes." He may nod when he utters "yes." He may affirm or deny by the less special language of pantomime when he cannot *use* for affirmation and denial the words of affirmation and denial which he can glibly utter. They are not, therefore, propositions to him. Nevertheless, this low degree of the utterances serves him. He utters "Yes," "yes," "yes," or "No," "no," "no," merrily, or he utters them sadly, when respectively glad or sorry. That is to say, although he has not the propositional use of "yes" and "no," there is that emotional service of them which other speechless patients have of their recurring jargon, words or phrase (1, 2, 3). His utterances of them in various tones are revelations to *us* of his varying emotional states. We must be careful not to give *such* utterances of "yes" and "no" the credit of being propositions. From the tones in which they are uttered, *we* may understand or guess how the patient is feeling; and with the conspiring aid of the then circumstances, we or his friends may often *infer* what he is thinking. But so we could by the tones in which the recurring jargon (1) or phrase (2) is uttered. Like smiles, they are, when so used, not signs for emotional states, but they are, or rather the tones of them are, *parts of* this or that emotional manifestation. On the other

hand, if the patient who generally uttered "no" at random, *used* "no" in a particular tone, in order to signify that he dissented, it would be speech, or at any rate of speech-value; and so it would be speech or of speech-value if a speechless man used his jargon with the same intention. The woman who uttered "ow" (p. 208) could express assent or dissent by the different tone she gave to it. The following is a striking case.

I have seen a patient who nearly a year before had become rapidly apoplectic. On recovering from this condition, he uttered only "low," but soon he uttered "no," and when I saw him he had nearly recovered speech. His articulation was defective, but his wife could understand what he said, not merely guess his meaning, and I could nearly always do the same. He could express himself in writing, and could read. For the moment using popular language ("*without prejudice*"), he had the mental power of speech, but had defect in the executive. But he very often uttered the word "no" when he meant "yes:" this is a very rare thing in the midst of so much recovery of speech. In reply to one of my questions, he uttered "No," "no." His wife said he meant "yes:" he nodded. Later in our investigation he uttered "no;" but his medical attendant, alive to his misuse of that word, said, "Do you *mean* 'no'?" The patient showed that he did by re-uttering it in a ceremonious, slow, decided, tone. Thus the patient *uttered*, in a tone to signify dissent, the word which, as usually uttered by him, would not have meant dissent.[1]

Here plainly "no" was not a proposition, but the tone it was uttered in was of propositional value—at least vocal pantomime. It matters not what trick or dodge (tone of voice, cardsharper's smile, &c.), be used to express assent or dissent, or to express any relation betwixt things; if so used, there is a proposition.

[1] Tylor says that in some languages, "especially in South-East Asia, rises and falls of tone to some extent, like those which serve us in conveying emphasis, question and answer, &c., actually give different significations. Thus, in Siamese, há = to seek, hā = pestilence, hà = five. The consequence of this elaborate system of tone-accentuation is the necessity of an accumulation of expletive particles to supply the place of the oratorical or emphatic intonation, which, being thus given over to the dictionary, is lost for the grammar."—'Primitive Culture,' vol. i. p. 153.

Some years ago I had under my care in the London Hospital a man whose sole utterance was "no," and something like "eh," which was "yes" to him. His wife told me he could make the children "behave when they were at the top of the yard" by shouting out "No," "no," "no," in an angry tone. These emotional utterances of his may seem to have some slight propositional flavour; he may have *used* "no" in an angry tone, not merely uttered it during vexation.[1] This, however, is doubtful. The patient could reply "no," but the slight degree of his power of expressing himself may be judged of by his way of getting his children to understand what he wanted. He would make one stand before him; she would guess one thing after another until, by quickness or by lucky accident, she guessed what he wanted, or until he knocked her down with his fist. I shall have to refer to this case several times, and shall therefore call the patient "Dow."

(*b*) In some cases of loss of speech there is a use of the words "yes" and "no," which is higher than a mere emotional service. The patient can reply with them. Here then is evidence that the so-called speechless man is not absolutely speechless; he propositionises by "yes" and "no." Now we come to a very important matter. In the case of "no," at least the use of that word does not in all patients reach the level of normal speech: or, speaking more correctly, the patient cannot utter that word in all the ways healthy people can. He may be, as aforesaid, able to reply "no" to a question requiring dissent, when he cannot say the word when he is told and when he tries. This has been observed and commented on by Sir Thomas Watson, in the last edition of his 'Practice of Physic.' I found it out when giving a clinical demonstration of "Dow's" case. I told the students that he could utter the word "no;" but, to my chagrin, when I asked him to utter it, there was nothing but an articulatory effort. However, on asking him the preposterous question, "Are you ninety years old?" the word "No" came out at once. Again I asked him to "say no;" his efforts were fruitless, but we readily got the word out

[1] If a speechless man retains the two words, it is reasonable to suppose that the emotions of fear and anger would, to speak figuratively, appropriate the negative one, and that emotions of joy and sympathy would appropriate the affirmative.

of him again by asking another question, which obviously required a reply of dissent. His difficulty was not from nervousness: his wife had found out, before I did, that he could not say "no" when he tried.[1]

I find that I have led Kussmaul to misunderstand me on this matter. This distinguished physician writes: "Jackson and Sir Thos. Watson have even found that aphasic persons, unable to reply 'no' to a question, have nevertheless been led to do so by suggestions designed to make them angry, e.g. 'were they a hundred years old, or a thousand?' or such like." The questions were not designed to make the patients angry, and did not make them angry. The patients I speak of could reply by "no" at any time. A very preposterous question was asked in order that there might be no possible doubt that a negative was required. And I submit that the rejoinder "no" to such a question by the patient "Dow" was a reply, that it was "no" as a proposition; that it was speech, although inferior speech (not incomplete speech); and that it was not "no" as a mere utterance, like an oath coming out in anger. This patient, as I have stated, did use the word emotionally when vexed; but not so in rejoinder to the preposterous questions I asked him.

(*c*) In many cases of loss of speech, the patient, besides having the emotional service and also the power of reply with the words, can say "yes" or "no" when told (he has the full use of) these words. It may be said that this third degree of utterance of the word is not speech; that it is uttering the

[1] The following is from the 'Hospital Reports' of the 'British Medical Journal' December 2, 1871, and refers to a case of loss of speech: ". . . She was told to say 'No,' and could not. Directly afterwards, Dr. Hughlings-Jackson, observing she had a book on her lap, asked if the patient could read. Hearing this, the patient herself looked up and said, 'No, no, no.' She was again told to say 'no'; she could not. The nurse, having observed this peculiarity in another patient, said, 'Are you a hundred years old?' The response was 'No,' with a smile. Once more the patient was asked to say 'No,' but again she failed. . . ." It must be added that in her ordinary "conversation" she sometimes said "no," when she meant "yes." In some cases there is no difficulty in saying "no." Thus there is in the London Hospital an old woman who can utter only the words "yes" and "no," and "titty," "titty;" and another old woman who has both these words and the utterance "I'm very well," or "Very well." Each of these patients can say "no," when asked. Hence it is admitted that some aphasics have the full use of these words.

word as an articulatory gymnastic "for the sake of uttering it," not *using* it as a proposition. There are weighty reasons, however, for drawing attention to the three degrees of utterance of this word. The inability to say "no," when told, with ability to utter it in reply and also emotionally, is one of the most important facts in the matter of affections of speech. I shall speak on this matter after considering analogous peculiarities.

To resume. In some cases called *loss* of speech there is not absolute loss. The utterances (1) and (2) and (3) are not exceptions; the utterances "yes" and "no," in reply, are exceptions, these words being *used* as propositions.

These exceptions are very significant. The man has lost all speech, except the two most general, most automatic, of all his propositions. They are indeed very significant exceptions to the empirical division into loss of intellectual and conservation of emotional language; for, even regarded superficially, they stand on the border ground. These words are used by healthy people, now one way, now the other; they are sometimes parts of emotional manifestations, and may then be combined with an ordinary interjection, as in "oh! yes," or be duplicated as "no, no;" here the second "no" at any rate is interjectional. They are at other times used with full and definite propositional intent to signify "this is so," or "is not so." The word "yes" may be used at the same time, both for sympathy and agreement, it being occasionally hard to say whether the intellectual or the emotional side is more visible. Similarly some movements are at once pantomimic and gesticulatory.

We hear these words used nearly purely emotionally, very often. A woman suffering from pleurisy and in great distress, replied "no" to a question, used the word propositionally, and then went on uttering the word as a vehicle of tone, "No, no, no," in just the same way as she had been uttering "oh!" before the question; that is, it served her emotionally. A healthy man, told suddenly a piece of startling news, cries "No!" using "no" not actually to deny the truth of the statement; indeed he does not use it as a proposition; it is an ejaculation of surprise, equivalent to the exclamation "Non-

sense!" or "You don't say so!" These so used are interjections, not speech, and take low rank in language, little above that of bodily starts, parts of common emotional language. At the best they are propositions, entirely subordinated to the service of an emotion.

Then propositionally "yes" and "no" give assent or dissent to anything whatever; they are the blank forms of, or stand for, all negative and positive propositions—are, as it were, propositions almost reduced to positive and negative copulas. From their almost universal applicability they are very frequently used; they are the most general, most automatic, and most organised of all propositions. They are then exceptions proving the rule; the patient has lost all speech except these two propositions, which are at the "bottom" of Intellectual Language and at the "top" of Emotional Language. In other words, the retention of these two words is not exceptional to the principle of Dissolution. The reader may, however, urge that the other recurring utterances are exceptional. I hope to show, later on, that they are not. At present, I only say that I believe them to represent what was, or to represent part of what was, the last proposition the patient uttered or was about to utter when taken ill.

(2) *Occasional Utterances.*

These utterances are rare, except that some patients swear very frequently. We shall make three degrees of these ejaculations:

(1) Utterances which are not speech;

(2) Utterances which are inferior speech;

(3) Utterances which are real speech.

(1) Under excitement, the speechless man may utter "Oh!" or "Ah!" More than this, he may swear, or utter certain nasty words used by vulgar people when excited. (We use the term swearing in the wide sense of what is popularly called bad language; of course religious commination is not considered.) The occasional utterance may be an innocent ejaculation, as, "Oh dear!" or, "Bless my life!" None of these utterances are speech;

they have no intellectual meaning. Moreover, the patient cannot repeat them when he tries; he "utters," but does not "say." This will remind the reader of what was said of some patients who can reply "no," but cannot say that word when they are told to try. The patient "Dow" uttered the word "damn," one night, when vexed on his daughter coming in very late. Her mother told me of their surprise, and that her daughter said she would stay out late every night to get him to speak. But he could not repeat the expression. I never heard of any utterance in his case but of "damn" on that occasion, and his recurring "no" and "eh" (yes).

As said above, it is not a question of oaths only, but of ejaculations in general (interjections simple or compound).[1] They are all parts of emotional language; their utterance by healthy people is on the physical side a process during which the equilibrium of a *greatly* disturbed nervous system is restored, as are also ordinary emotional manifestations. (All actions are in one sense results of restorations of nervous equilibrium by expenditure of energy.) In some people oaths and vulgar interjection have become very deeply automatic; some people swear largely along with their ordinary unexcited speech, perhaps to give emphasis to commonplaces. In these people the oaths are almost as automatic (their nervous arrangements being strongly organised) as smiles and frowns; they are, so to speak, "detonating commas." No wonder that they are Occasional Utterances when these patients are speechless. Few women swear, but their ejaculations of surprise or vexation (feminine oaths), as "Oh! dear," "Dear me!" "How very tiresome!" belong to the same category. The aphasic woman, whose recurring utterance was "me, me," &c., once ejaculated, "God bless my life!"

[1] I take the following from an unsigned review in the 'Journal of Mental Science' for April 1878, p. 125: "The value of swearing as a safety-valve to the feelings, and substitute for aggressive muscular action, in accordance with the well-known law of the transmutation of forces, is not sufficiently dwelt on. Thus the reflex effect of treading on a man's corn may either be an oath or a blow, seldom both together. The Scotch minister's man had mastered this bit of brain-physiology when he whispered to his master, who was in great distress at things going wrong, 'Wad na an aith relieve ye?'"

It has been said that he who was the first to abuse his fellow-man instead of knocking out his brains without a word, laid thereby the basis of civilisation.

(2) There are occasional utterances which are real speech, but inferior speech. This remark may be indefinite, but illustrations will show what is meant.

I saw, in consultation, a patient who had the recurring utterances "no" and "what." This patient was heard by his doctor to say "Wo, wo!" when standing by a horse. This patient once uttered "That's a lie," which is an expression often used by vulgar people as a verbal missile, that is emotionally rather than propositionally; it therefore comes under No. 1. The utterance "wo, wo!" is the one I wish to draw attention to now. "Wo, wo!" is a proposition to those who use it, if not to animals; it means "stand still."

A woman who could only utter the phrase "Yes, but you know," once said "Take care!" when a child was in danger of falling. A patient of Trousseau's said "*merci*," when a lady picked up his handkerchief. A patient of mine would, besides swearing when vexed (No. 1), say "Good-bye," when a friend was leaving him.

The man who said "Wo, wo!" could not repeat it; and the lady could not repeat "Take care!" The friends of Trousseau's patient thought he was beginning to speak; but he could not repeat the word. My patient could never say "Good-bye," except under the appropriate circumstance; his daughter had found this out herself.

The following is from a communication made to the *Lancet*, May 18, 1878, on this patient's case. Several different phenomena are mentioned; they are all of the same order, in so far that they show conservation of automatic with loss of voluntary action.

"I have seen a patient who usually sat up in his room, whose face looked intelligent, who was cheerful and merry, and who seemed to understand all that I said to him, but who could not put out his tongue when he tried. His daughter remarked that he could put the tongue out, as she expressed it, 'by accident,' and added, as an illustration of her meaning, that when any one was leaving him he could say 'good-bye,' but that he could neither put out his tongue nor say 'good-bye' when he tried. He could say 'yes' and 'no' at any time; and, using the lady's expression, could say 'good-bye,' 'well,' 'never,' *by accident*.

She further remarked that the patient would sometimes swear. He uttered the short explosive word which is so much in favour with English swearers, but he could not, she said, repeat the word when he tried. She asked him to utter the explosive sound when I was there, saying it herself for him to imitate. He laughed, and shook his head."

Admitting the utterances (No. 2) to be exceptions, we have to note that, as exceptions, they are significant. They are true speech, but they are inferior speech. Superiority in speech does not mean number of words, nor even solely precision of application, but precision of application to new relations of things, that is, in effect superior speech is accurate speech on complex[1] matters. We do not find that the loquacious person speaks precisely, except on the most familiar things; on novel things he fails greatly. The "faculty" of speech is not, as popularly supposed, highly developed in him. The utterances are well organised; they were prompted—to speak popularly, helped out—by their special circumstances. They are only in degree less significant than the Occasional Utterances (1), or than the Recurring Utterances (4). Besides this, the inability to repeat them is to be carefully borne in mind.

(3) I have records of still higher degrees of utterance by one speechless patient. A man, for several months under my care in the London Hospital, was absolutely speechless. He never *uttered*, much less spoke, anything but "pooh," "pooh," so far as I or the students or the nurses knew. But I was told by his friends of three utterances. Once, when he had had enough bread-and-butter, he said "No more." This, however, is only a degree of speech on a level with those in the just-given illustrations (2). But I was told that one day the patient said,

[1] Although of necessity we take type cases, we not only consider what we call exceptions to the type, but in actual practice we consider individual peculiarities. What is well organised in one person is not so in another; when we say that the more automatic, more organised, &c., remains, we mean what is more organised in this or that patient. The qualifications to be understood in using such expressions as "*the* concept," "*the* English language," "*the* environment," need not be pointed out. In such expressions as "from *the* special to *the* general," "from *the* complex to *the* simple," the obvious qualifications must be kept vividly in mind. When we speak of complexity of any actions, we do not mean any sort of abstract complexity. A man in delirium goes through very complex manipulations of his trade, but they are not complex *to him*.

with difficulty of articulation, "How is Alice [his daughter] getting on?" A third utterance was, I think, as high, if not still higher, in speech. His son wanted to know where his father's tools were. In reply to his son's questions, the patient said, "Master's." Although here is but one word, where in health there would have been a sentence, there is a proposition; it told his son where the tools were as fully as the most elaborately worded and grammatically complete sentence would have done. It was far higher than the most elaborate oaths, and higher even than such utterances as "no more," "good-bye," "very well," &c. Once more I would urge that speciality in speech ("high speech") is not simply an affair of number of words, nor simply of complexity of their arrangement. We have to consider precise adaptation to special and new circumstances: "master's" did not come out upon a common and simple occasion, like "good-bye;" it was definitely uttered to signify a very special relation, moreover a new relation. Granting, for the sake of argument, what, however, I do not know, that the man had in health replied scores of times to the same question by that word, or by a fuller proposition containing it, it was specially used for a new occasion, under, that is, very new circumstances. The father had left his work, would never return to it; was away from home; his son was on a visit, and the question was directly put to the patient. Any one who saw the abject poverty in which the poor man's family lived would admit that these tools were of immense value to them. Hence we have to consider, as regards this and the other occasional utterances, the strength of the accompanying emotional state. We shall consider the influence of strong emotions, which imply great nervous tension on the production of these utterances, later on.

I used to receive reports of these utterances and sayings by speechless patients with great incredulity, and so I find have others done. One of my most intelligent pupils, to whom I was speaking of such utterances, told me that the wife of a speechless patient, with much indignation at his (the student's) incredulity, affirmed that her husband (otherwise speechless since his attack) uttered before he died, "God bless you, my dear!" This utterance, if largely emotional, was a painfully

appropriate one from a dying man to his wife. He may have uttered it interjectionally scores of times when well, and have *said* it when dying; really *meaning* that God should bless his wife. Under some circumstances truths that have died down into truisms become alive again.

The Communist orator who began his oration by "Thank God, I am an Atheist!" used "thank God!" as a mere expletive: even when this phrase is uttered devoutly, it is often more emotional than propositional; but in some states of mind it doubtless is *said* with full propositional intent. Much poetry, in prose and verse, nowadays seems to be an attempt to show the truths of what have become uninfluential truisms.

These utterances naturally surprise the friends of speechless patients. A patient, fatally ill, unable to tell what she wanted (this patient had not entirely lost speech), surprised her sister by exclaiming, "Surely you must know what I mean?" after that she said nothing intelligible. A patient under my observation in the London Hospital could utter many words, but his oaths and other ejaculations were alone properly uttered; a patient in the next bed felt insulted on being asked to note what the patient uttered. Naturally he would feel that a man who, when asked to write, ejaculated, "What's all this bloody nonsense about?" could talk if he liked.[1]

Gairdner had an aphasic under his care in hospital, and wishing to learn something as to the patient's general condition, asked another man in the ward what *he* thought of him. "I think a guid whuppin wad be the cure of him." On Gairdner remarking that the patient could not speak, the man replied, "Na, but he swears whiles;" evidently believing that the poor

[1] Dow's wife told me that the neighbours were very unkind; they said it was all nonsense about his being unable to talk, for why did he not write? They could not be expected to know that if speech goes, writing goes—expression in writing is meant; Dow copied a good deal, and could sign his name without copy. Had they heard Dow utter "Damn," because his daughter came in late, they could have felt the correctness of their opinion of his case to be demonstrated. In general the laity cannot be expected to know that swearing, &c., may persist when speech proper is impossible, and certainly not that a higher kind of utterance may persist when the patient is fatally ill. No doubt many apoplectic persons found in the streets are locked up for drunkenness because the policeman does not know that swearing is a very automatic process, which can persist under conditions produced by fatal brain lesions as well as by drink.

fellow was shamming. The aphasic died, and cancer of the brain was found at the necropsy.

In some cases of speechlessness an elaborate utterance comes out of which we cannot guess the meaning. The following case is an illustration of this and also of other utterances. A patient under the care of Dr. Martin,[1] in St. Bartholomew's Hospital, could only utter the word "yes." The Sister of the ward (a very intelligent lady) remarked that he uttered this word when he meant "no;" moreover, she said he often nodded when he meant "no." One of the nurses told her that the patient once, in words, asked for beer; but the Sister remarked, "I don't believe this, as I was constantly with him, and never never heard him say anything." I think it very likely he did, under strong excitement (active desire), get out a proposition to that effect. It may be said that there could be no excitement about so small a matter. But it is no small matter to many hospital patients. Some will leave the hospital if they do not get beer. But the utterance I wish to draw attention to is the following. His wife said that all she ever heard him utter beyond "yes," was "Five nights, six nights, seven nights, and then five nights out of seven." What this meant she could not guess.

To resume once more. There are three exceptions to the statement that our "Speechless" man is absolutely speechless. He may have permanently the utterance of the words "yes" and "no," and the full use of them: their use as speech. On the other hand, we have noted that a patient may have only the emotional or interjectional use of them, and that when he has, more than this, the ability to reply with them, he may be unable to say them when told. And where there is the full use of them, we have to bear in mind that they are the most general of all propositions. Then he has *occasionally* some inferior speech, and as I believe this to be effected by the right half of his brain, I admit that these occasional utterances show, as do "yes" and "no," some power of speech during activity of that side. Anyhow, they show that the patient retains organisations for some words somewhere in his nervous system.

[1] Dr. Martin kindly allowed me to see this patient and to report it, 'Lond. Hosp. Reports,' vol. iv. p. 365.

There is no demonstration by these cases that the patient retains organisations for any other words than those he actually utters. But is it a likely thing that Trousseau's patient, who said "*merci*," when a lady picked up his handkerchief, had just that word or a few such words left? A fire occurred in the street opposite one of my wards in the London Hospital: a speechless patient of mine cried out "fire!" Is it not a grotesque supposition that this woman retained only the word "fire"? Moreover, those who say a patient tried to repeat any of his occasional utterances are tacitly admitting that the words of those utterances are revived in him; otherwise the word tried has no meaning. There is demonstration by other means that the speechless patient retains a full service of words; he understands what we say to him. At any rate, the utterances spoken about show that there is retention of some words, if only a fragment or so in each case. Some of them also show that there is not only retention of some words, but of some speech, by the right side of the brain. The division we made (vol. i. p. 319) was not that the left half of the brain serves in speech, and the right in receiving speech and in other ways, but that "nervous arrangements for words used in speech *lie chiefly* in the left half of the brain," and "that the nervous arrangements for words used in understanding speech (and in other ways) lie in the right also." It is believed that the process of verbalising and every other process is dual, but that the more automatic a process is, or becomes by repetition, the more equally and fully is it represented doubly in each half of the brain. But the utterances show too, for the most part, that the speech possible by the right side of the brain is inferior speech. In nearly all cases it was well organised automatic or "old," and nearly every utterance required a special occasion, was, to speak popularly, surprised out of the patient by a sudden accustomed stimulus. And it is to be borne in mind that the patient cannot repeat, say voluntarily, what he thus utters. So far these exceptions are exceptions proving the rule.

It has been admitted, however, that occasionally there is an utterance of high speech-value. This exception will be considered after a while.

VOMITING IN CONNECTION WITH CEREBRAL DISEASE.

BY D. FERRIER, M.D., F.R.S.

THE conditions under which vomiting occurs are so numerous and so diverse that it is difficult to understand how the same mechanism can be set in action in so many different ways. In attempting to explain the various symptoms met with in connection with cerebral disease, vomiting is a symptom which demands special attention, on account of its frequency and grave significance. If the present state of our knowledge does not supply us with data for a definitive solution of the problem, it may, however, be useful to inquire whether we can assimilate vomiting from cerebral affections with vomiting due to other causes, apparently more simple, because more familiar.

Like all reflex actions, vomiting must be looked upon primarily as an indication of adaptive reaction on the part of the organism. The only condition, however, in which this adaptation is at once clearly evident is in presence of morbid irritation of the stomach or upper part of the alimentary canal—vomiting being a reflex reaction adapted for the expulsion of substances causing such irritation. Though no irritating substances should be actually present, yet it is physiologically the same thing, if similar irritation should exist from any cause. Hence vomiting from morbid irritation of the stomach, however produced, is readily explicable.

Without actual irritation of the fauces, or of the stomach, however, vomiting may be caused by unpleasant odours or tastes.

Whether there is any close anatomical relationship between these nerves and the sensory nerves of the stomach, or whether

the solidarity subsisting between smells, tastes, and substances congenial to the stomach is merely the product of association, is a question which might be of interest to discuss; but without entering on this, we may take it as a fact that certain smells and tastes are practically the same thing as gastric irritants—the senses of smell and taste being merely the advanced guard of the stomach. The connection, it is true, may be broken through by effort and habit, yet this does not invalidate the general rule.

Under the same category as actual unpleasant odours or tastes may be placed the vivid realisation in idea of the same.

For the ideal smell or taste is merely the central reproduction of precisely the same kind and quality of impression, as result from the actual application, at the periphery of the olfactory or gustatory apparatus, of the evil-smelling and evil-tasting substances. All the other physical concomitants tend likewise to be reproduced, and hence vomiting may occur if the ideal reproduction is sufficiently vivid.

Closely allied to vomiting from such causes is the occurrence of vomiting from the feeling of emotion or Disgust.

The facial expression of disgust coincides with the natural reflex movements of the nose and mouth indicative of repulsion of unpleasant odours or tastes. Disgust and its facial expression are no doubt primarily founded on objects repulsive to taste and smell, and therefore unfit for food; but by association, various other secondary objects have been engrafted on to it, related at a greater or less distance with things abhorrent to the stomach.

Hence disgust can be excited and vomiting actually produced by such things as actual or ideal contact with cold slimy things, or with filth, putridity, or messes in general. Disgust reaches its acme by processes of sympathetic realisation when we see others dealing with things the very sight of which is repulsive to us. The sight of vomited matters, and still more the sight of a person vomiting, especially if there is any tendency to sickness already existing, are sufficient to bring matters to a crisis.

In all these cases there is actual irritation, conditioned either

centrally or peripherically, of the sensory nerves of the stomach, or their physiological associates.

Though vomiting is specially serviceable in expelling irritants from the stomach and upper part of the alimentary canal, yet, as we see in stercoraceous vomiting, it may prove efficient in emptying the intestinal canal. Hence the occurrence of vomiting in connection with irritation of the alimentary canal, otherwise than in the stomach, may be brought under the same law of adaptive reaction. Vomiting therefore in connection with strangulated hernia would thus be accounted for.

But vomiting from direct irritation of the alimentary canal forms only a small proportion of the conditions under which it may be manifested. Vomiting is of frequent occurrence in connection with irritation, variously induced, of the abdominal and pelvic viscera. We find it with diseases of the liver, more especially during the passage of a biliary calculus; with inflammation of the kidney, and specially during the passage of a renal calculus; with diseases of the spleen, and pancreas; and in a very marked degree in connection with irritation of the uterus and ovaries. Irritation of the peritoneum also, and of the omentum, as in cases of omental hernia, are frequently associated with vomiting.

In vomiting so caused it is not easy to see any such adaptiveness as is clearly manifest in relation with direct irritation of the stomach or alimentary canal. A closer examination, however, may discover in vomiting under some of these conditions a reaction possibly beneficial to the organism.

Thus in the case of an impacted biliary calculus, the violent compression and concussion of the abdominal viscera during the act of vomiting may expedite its passage and remove the source of irritation. Similarly also in the case of a renal calculus. And that the uterus may in this way expel an irritant from its interior, such as retained menstrual fluid, is a fact of which it would be easy to give examples. But vomiting due to irritation of the abdominal or pelvic viscera and their investments, is just as often, if not more so, hurtful than beneficial. So that if we were to look upon vomiting as in all cases the indication of a *vis medicatrix naturæ*, we should have

to admit that this is capable of making gross and pernicious blunders.

In vomiting so induced, we may see rather an illustration of the laws of irradiation which have been established in reference to spinal reflex actions. The reflex action consequent on irritation of any particular spot is at first more or less definite, and limited in reference to the source of irritation; but if the irritation is stronger and more continued, or if the reflex excitability is heightened, there is a tendency to irradiation and the discharge of other movements. This irradiation follows certain definite lines; those movements being first discharged which are most closely allied and symmetrical than others at a greater distance, until ultimately a general convulsion may result.

Though coughing is primarily adapted for the expulsion of irritants of the air-passages, and is conditioned mainly by irritation of the sensory nerves of the lungs, yet it may be set in action by irritation of closely related parts, as by irritation of the pleura, in which case no useful purpose can be effected, but rather the reverse. Similarly sneezing, though primarily conditioned by irritation of the nasal passages, may be excited by a bright light, apparently by mere contiguity.

The anatomical conditions of the innervation of the viscera by closely-connected plexuses of the vagus and sympathetic nerves are such as to render irradiation of impressions from one point to another more than usually easy. Numerous channels are open for the transference of impressions; hence Synæsthesiæ or sympathetic sensory neuroses are common, and the same facility exists for the excitation of synkineses, or sympathetic motor reactions. The nearest reflex reaction is the act of vomiting, but coughing may also occur, or, if the irritation is great, general convulsions may be induced.

We may therefore regard vomiting in connection with visceral irritation mainly as an indication of irradiation, and the excitation indirectly of the reflex reaction which is specially characteristic of irritation of the stomach and alimentary canal.

But when the irritation is of an intense character, another factor has to be taken into account in addition to the mere

fact of irradiation, viz. the sensation of pain. For intense pain may of itself cause vomiting, apart from any particular seat. As a general rule, however, vomiting occurs most frequently in connection with intense pain in organs and parts whose sensibility under ordinary conditions does not distinctly form part of our consciousness.

Owing to the intense pain which accompanies the passage of a biliary or renal calculus, vomiting here is not a simple case of irradiation. But that vomiting may result from irradiation of visceral irritation not involving pain is evident from the vomiting of pregnancy due to uterine irritation. Vomiting caused by pain leads to the consideration of another class of conditions in which vomiting is frequently seen, independently of direct irritation of the alimentary canal or indirect irritation by irradiation. Vomiting is common in concussion of the brain and in shock from severe traumatic injuries. Concussion of the brain, and shock with syncope are essentially the same. The main features in both are a temporary annihilation of consciousness, with a more or less enduring state of the system in which intense depression of the circulation is the most marked phenomenon. There is great muscular prostration, the face is pale, the skin cold and covered with a clammy perspiration, the temperature is lessened, the pulse almost imperceptible, and the vascular tension almost nil. In this state there are frequent attempts at vomiting or actual sickness.

Shock has been shown by Dr. Lauder Brunton ("The Pathology and Treatment of Shock and Syncope"—'Practitioner' vol. xi. p. 241) to depend chiefly on dilatation of the abdominal blood-vessels.

The most efficient cause of shock is traumatic injury of the abdominal viscera. A smart tap on the intestines of the frog, as in Goltz's experiment, almost annihilates the circulation by causing reflex dilatation of the abdominal vessels. The abdominal vascular area is so great that when fully dilated the abdominal vessels can accommodate nearly the whole of the blood of the system. In such case, even though the heart may continue to beat, little or no blood flows through it, and therefore the circulation is only not at a complete standstill.

The same cause also usually induces reflex stoppage of the heart, and thus we have shock combined with syncope or temporary annihilation of consciousness, but the syncope may pass off and the symptoms characteristic of shock continue.

The accumulation of blood in the abdominal viscera is sufficient to account for the pallor of the skin and the other phenomena indicated, for though the other vessels might also be dilated, yet they would be drained of their contents.

The phenomena of syncope and shock would seem to show that vomiting may be induced by conditions causing great lowering of the blood-pressure and depression of the heart's action.

This is further borne out by the effects of copious hæmorrhage. Venesection *ad deliquium animi* was a frequent cause of sickness and vomiting.

The substances which act most powerfully as emetics, apart from those which have a directly irritant effect on the stomach, have an intensely depressant action on the circulation, and produce all the phenomena characteristic of shock. Vomiting is almost an invariable accompaniment of the toxic effects of the cardiac poisons. Some of these, like veratria, may also exercise a directly irritant action on the alimentary canal, but in others the vomiting is out of all proportion to the irritant effects which are discoverable.

From these considerations it would appear that whatever causes great depression of the circulation may cause sickness and vomiting, and thus we may bring into relation with each other conditions which at first sight seem to have nothing in common. Vomiting, as has been observed, is frequently seen in connection with great pain. A blow on the testicle, a loose cartilage suddenly coming between the articular surfaces, a dislocation, a traumatic injury of the eyeball, and many other forms of pain have been known to cause nausea and vomiting.

The experiments of Mantegazza (Schmidt's 'Jahrbücher,' 1867, 133) show that intense pain causes a depression of the circulation similar to what is seen in shock and syncope; and Weir Mitchell has related a number of cases ('Injuries of Nerves,' p. 138) in which shock resulted from gunshot wounds of nerves. From these latter it would appear, however, that

the shock is not in proportion to the consciousness of pain, but to the physiological reflex effects of the injury itself.

The conditions which give rise to the consciousness of intense pain are such as to induce the phenomena characteristic of shock in a greater or less degree, and therefore also nausea and vomiting. The intense depression of the circulation is the common point of agreement. The physical effects of intense mental pain, such as is exhibited in the emotions of fear and terror, are precisely the same as those of intense bodily pain, and act through the same channels. Hence nausea and vomiting may result under essentially the same conditions. Suffering in others has often a sickening effect on bystanders. This is frequently met with in those who witness a surgical operation for the first time. The suffering of the patient, or the supposed suffering, is realised sympathetically by the onlooker.

In addition to these various causes of nausea and sickness there is another condition in which these symptoms are specially apt to occur, viz. in connection with vertigo and disorders of equilibration. The sickness may in part be due to the terror which arises from a sudden sense of insecurity, yet there are many considerations which point to a direct relation between the viscera and the centres of equilibration.

Thus visceral disturbances are very often associated with vertigo. And as visceral disturbances, mechanical or otherwise, induce vertigo, so the motor inco-ordination, of which the sense of giddiness or vertigo is only the subjective accompaniment, whether due to central causes or to peripherical affections, as in labyrinthine vertigo, tends, by a process similar to irradiation, to produce visceral commotion, and with it nausea and vomiting.

As regards the immediate physiological antecedents of vomiting in connection with great depression of the circulation, we can say nothing very definite as to whether there is direct irritation of the vagus roots in the so-called vomiting centre, or indirectly from the condition of the heart and blood-vessels, of which the abdominal blood-vessels are the most important.

But that irritation of the vagus, centrally or peripherically, is the ultimate fact may be fairly assumed, and thus we may

see a closer relation between vomiting caused by irritation of the stomach and alimentary canal, and vomiting under the various conditions described than seems at first sight to exist.

It is possible also to see in vomiting in connection with depression of the circulation a reaction distinctly beneficial to the organism.

Thus in cases of vascular dilatation in the splanchnic area, the violent compression and concussion of the abdominal walls in the act of vomiting serve to propel the blood onwards, and thus raise the blood-pressure. It is observed in concussion of the brain and shock that vomiting is generally the precursor of recovery, the blood-pressure rising and the circulation re-establishing itself. Marshall Hall also, in describing the effects of loss of blood, calls attention to the fact that "the state of syncope is often relieved, for a time, by an attack of sickness and vomiting, immediately after which the patient expresses himself as feeling better, and the countenance is somewhat improved, the breathing more natural, and the pulse stronger and more frequent" ('On the Effects of Loss of Blood,' p. 11).

With this brief review of the chief conditions under which vomiting is met with, apart from affections of the brain, we may now inquire in what relation, if any, cerebral vomiting stands to them.

Many grave organic diseases of the brain run their course without vomiting as a symptom. The forms of cerebral disease in which vomiting is especially met with will be found on examination to correspond with those in which headache is also a prominent symptom, and therefore, as I have previously ('BRAIN,' Part V.) endeavoured to show, in cases where there is irritation of the nerves of the cerebral membranes by inflammation, excessive tension, and the like.

Hence vomiting is especially manifested in connection with meningitis and cerebral tumours. Vomiting occurs in affections of this kind altogether independently of the position of the lesion. There is no evidence of any value in support of Budge's assertion that vomiting is more frequent with disease of the right hemisphere than the left. Nor does there seem better evidence for his view that the right optic thalamus and

corpus striatum have special relation to the stomach, than his view that the cerebellum has special relation with the testicles.

There is no evidence, physiological or pathological, that direct irritation of the substance of the brain, apart from irritation of the cerebral membranes or other complications to be mentioned, is capable of exciting vomiting. Vomiting and headache generally go together.

It would not, however, be correct to assert that vomiting in cerebral disease is proportional only to the intensity of the pain. That the intensity of the pain alone, particularly of the agonising character frequently seen in cerebral tumours, may be sufficient to cause sickness and vomiting, would be in accordance with the effects of intense pain in other regions, and would be explicable in the same way.

But we may observe vomiting of a pathognomonic character in the early stages of tubercular meningitis, before pain has reached any marked degree of intensity. The headache and vomiting do not stand in direct relation to each other.

Frequently, in fact, pain in the head and vomiting alternate with each other, the headache giving way to vomiting, and *vice versâ*.

We have therefore reasons for concluding that vomiting may be excited by a degree of irritation short of that necessary to cause intense pain with its constitutional depression. In fact the comparative ease of cerebral vomiting without the marked nausea and depression which accompany sickness from intense pain, would seem to indicate a specially close relation between the sensory nerves of the cerebral membranes and the centres concerned in vomiting, the resemblance between cerebral vomiting and uterine vomiting being closer than between the former and vomiting from a renal or biliary calculus.

The facility of irradiation of visceral irritation and the excitation of vomiting has been already alluded to, but the question is whether there is such an intimate association between the sensory nerves of the cerebral membranes and those of the stomach as to account for cerebral vomiting by a similar process of irradiation.

As regards the physiological association, we have sufficient evidence in the connection between headache and gastric disturbances, so constant that the head is in a measure the sensory index of the stomach. This in all probability is to be explained by the close anatomical relationship between the nuclei of the vagus and trigeminus in the medulla oblongata, rendering the transference of impressions from one nerve to the other comparatively easy. Hence vomiting from irritation of the cerebral membranes may be brought under the same principle of irradiation as vomiting from visceral irritation not directly affecting the stomach. As in this case, so in irritation of the cerebral membranes, there is a synæsthesia of the stomach, and vomiting in consequence. This mutual synæsthetic relationship between the nerves of the stomach and cerebral membranes seems to me to account most satisfactorily for the phenomena of migraine or sick headache. Here we have an association of intense headache and vomiting, the two symptoms often alternating with each other, and also very frequently certain remarkable paræsthesiæ or even parakineses, chiefly unilateral, which may be accounted for by concomitant affection of the cortical centres in immediate relation with the seat of irritation in the cerebral membranes. The clinical history and etiology of migraine accord well with the hypothesis that it is essentially a neurosis of the cerebral membranes.

While we may ascribe the great majority of cases of cerebral vomiting to irradiation of irritation of the nerves of the cerebral membranes, or to the physical effects of acute pain, there are some cerebral affections in which possibly another cause may be operative. It is generally believed that vomiting is more especially associated with lesions of the cerebellum and corpora quadrigemina. Diseases affecting the centres of equilibration might be accompanied by sickness more through the vertigo induced, than from mere irritation of the cerebral membranes.

In the facts recorded, however, it is not easy to eliminate what may be due to the lesion as such, and the causes operative here as elsewhere. For the anatomical relations of the posterior fossa of the skull are such as to allow of irrita-

tion of the cerebral membranes often of a very definite and circumscribed character. Should vomiting be proved to be present in such cases apart from irritation of the membranes, we might account for it by disturbances of equilibration and the concurrent vertiginous sensations. But, apart from these circumstances, irritation of the cerebral membranes seems to afford a sufficient explanation of most cases of cerebral vomiting.

Critical Digests and Notices of Books.

Luciani and Tamburini on the Functions of the Brain. The Psycho-sensory Cortical Centres. 'Sulle Funzioni del Cervello. Ricerche Sperimentali dei Professori Luigi Luciani e Augusto Tamburini. Seconda Comunicazione: Centri Psico-Sensori Corticali. 8vo. pp. 78. Reggio—Emilia. 1879.

In this communication the authors confine themselves to an experimental inquiry into the centres for visual and auditory perceptions. The method of investigation consists, as before, in exposing, under chloroform, certain convolutions by the trephine, examining the electric excitability, and thereafter carefully removing the cortex so as to avoid hæmorrhage as much as possible.

I. In their historical introduction, the authors mention that Flourens, the founder (1842) of the scientific inquiry into cerebral localisation, held that the brain is an organ, physiologically one, performing all its functions both as a whole and in every part. According to his view, therefore, partial mutilations of the brain would weaken the power of performing all functions, and when complete or great removal was effected, all kinds of psychical activity would be annihilated. Hearing and sight would therefore disappear on removal of the mass of the brain. This view was combated by many, and Schiff contended (1858) that not even removal of a complete half of the cerebrum had any influence on vision. Meynert (1866) by following the course and termination of the hemispherical fibres of the auditory nerve, came to the conclusion that to the cortical substance of the convolutions of the fossa of Silvius belonged the function of receiving impressions of sound.

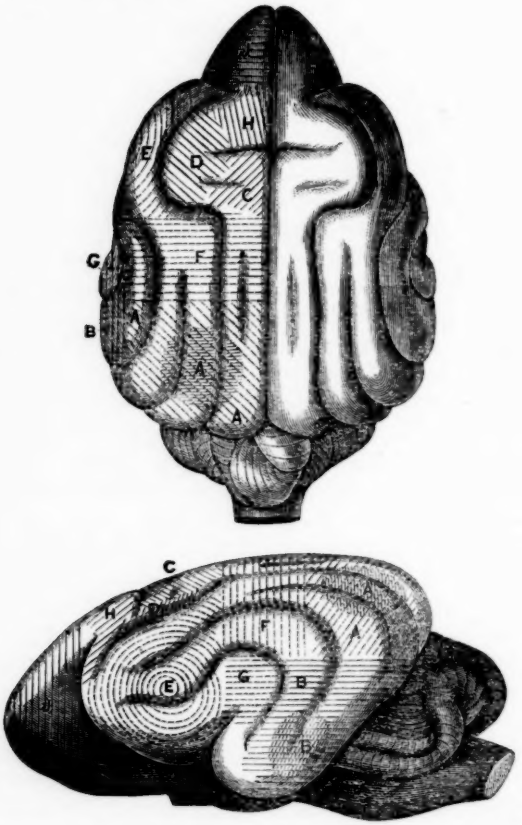
Charcot has recently said that the cortical sensory centres would probably be found in the temporo-sphenoidal and occipital lobes. In 1874 Hitzig first pointed out briefly that lesion of the posterior lobe of the brain can produce blindness of the opposite eye.

By means of the water jet, Goltz showed that when an important part of the cerebral cortex is removed, temporary blindness of the opposite eye ensues. This result follows (he says) even after removal of the so-called motor zone of Hitzig. Although the animal recovers, it does not do so completely, the animal never quite recovering the power of appreciating distance and of distinguishing the nature of objects, and being impassive. He does not deny that disturbance of other senses than sight (hearing, for instance) may ensue from such a lesion, but does not say he has completely investigated the matter.

Ferrier showed that a much larger portion of the cerebral cortex than Hitzig had succeeded with was excitable, and he attempted to localise (among other sensations) hearing in the superior temporo-sphenoidal convolution, and sight in the angular gyrus, or *pli courbe* of monkeys. In dogs, the visual centre, according to Ferrier, is located in the parietal portion of the second external convolution, while the centre for hearing is defined as the upper and posterior portion of the third external convolution. Ferrier confirms his view of these localisations, drawn in the first instance from the occurrence of certain (motor) reactions on irritation of the centres, by proceeding thereafter to remove the centres. He maintains that unilateral removal of the angular gyrus is followed by complete, but not permanent, blindness of the opposite eye, while after bilateral destruction it is complete and permanent. In the case of dogs and cats, Ferrier's experiments are less complete, and he quotes only one experiment where he destroyed the parietal portion of the second external convolution in a cat, with the apparent result of complete blindness in the opposite eye. He believes that the temporo-sphenoidal lobes in monkeys, and the upper and posterior part of the third external convolution in dogs, are centres of *auditory sensations*, and believes that removal of these centres is followed by annihila-

tion of the sense of hearing, though admitting the difficulty of settling the question.

Hermann Munk has experimented since Ferrier, and does not altogether agree with him.



Functional localisations of the cerebral cortex of the dog, according to Munk. A. Visual sphere. B. Auditory sphere. C. Sphere of the posterior limbs. D. Sphere of the anterior limbs. E. Sphere of the head. F. Sphere of the eyes. G. Sphere of the ears. H. Sphere of the neck. I. Sphere of the trunk.

According to this writer, the brain of dogs may be divided into an anterior or motor portion, and a posterior or sensory,

by a vertical line passing from the end of the fissure of Silvius to the falx. Destruction of the centres in front of this line induces lesions of motility, while removal of those behind it does not produce any motor phenomena. If the lesion affects the occipital lobe towards its posterior superior extremity, (A, Fig. p. 236) certain characteristic disorders of the visual function ensued, which Munk explains as the effect of amnesia of the visual images, and which he terms *psychic blindness;* when, on the other hand, it affects the temporal lobe near its inferior extremity (B, Fig. p. 236) special changes of the auditory functions are manifested, which he explains as effects of (amnesia) loss of memory of auditory images, and terms *psychic deafness.* If the ablations do not involve the said segments, but other areas placed just in front of and below them, limited disorders follow which last only three to five days, and which Munk entirely overlooked in his first researches.

The memory of single visual (and presumably also of auditory) images would, according to Munk, have a fixed and determined site in the cerebral cortex. He believes, in fact, that he has shown that a dog on one occasion lost memory of all visual images, except that of the dish from which it was accustomed to drink; and that another dog remembered only the gesture in response to which it had been previously accustomed to extend its paw. His explanation is, that in the case of the first, all the parts of the cortex had been removed except that part which retained the memory of the dish; and in that of the second, all those parts except that which retained the memory of the manual gesture. Munk asserts further that the forgetfulness of visual and auditory images passes off, and in four to six weeks the animals experimented on cannot be distinguished from those perfectly sound. He compares such animals with those newly born, and says that, like these, they gradually lay up ideal images of auditory and visual sensations. The question then arises, Where are such representations deposited, since the physical substratum naturally set aside for them has been removed? Munk says, in the parts of the brain surrounding those parts. He has not been able definitely to localise the new representations, however, because all his animals died of most acute meningitis in his efforts to remove

the surrounding centres. So also did all the animals from whom he removed the visual centres plus a radius of twenty millimetres surrounding them, in order to determine when the normal function failed to be restored. In the case of two dogs, he succeeded in removing the whole cortex of the left hemisphere of the occipital lobe (A A A). This was followed by complete blindness (not simple psychic blindness) of the right eye, which passed off after some time. He could not clearly determine whether the mnemonic images returned; and when he tried to remove the corresponding portion of the right occipital lobes, both the dogs died.

In monkeys, according to Munk, the visual sphere is localised in the cortex of the occipital lobes, and not in the angular gyrus as maintained by Ferrier. The extirpation of a circular area (10–15 mm. in diameter) in the occipital lobe produces limited disturbances of vision; the animal seems to have lost the memory of visual images for certain objects, preserving those of others. If the entire cortex of the convex surface of the occipital lobe is destroyed, the animal becomes hemiopic or cortically blind in the halves of both retinæ on the same side as the lesion, on which half of the retinæ the animal does not recognise objects, though it does so perfectly on the opposite half. If the cortex of both occipital lobes is removed, the monkey becomes entirely blind, habitually quiet and apathetic, and when compelled to move, it stumbles against every obstacle in its path. After a certain time, vision partially returns, so that the animal can walk about slowly without stumbling. If certain portions of the cortex at the margin of the convexity of the occipital lobe remain intact, a greater amount of recovery may occur, and hemiopia in the halves of the retinæ corresponding with the less completely destroyed side can be made out. Monkeys are essentially, therefore, different from dogs, in whom Munk has never been able to make out bilateral visual disorder after unilateral lesion.

Munk's latest view of the motor region of the cortex has been harmonised with the view just given of the visual and auditory functions. He looks upon what is generally considered the Motor or Psycho-motor region as sensory in the widest sense, or as the seat of the sensory perceptions of the body, and

of the images or sensory concepts which are thence derived. Destruction, therefore, of this sphere produces psychic paralysis of the said perceptions, and loss or forgetfulness of the sensory images there resident, partial or complete, according to the amount removed. Motor disorders on this view are the results pure and simple of abolished cutaneous muscular and nervous sensibility. He makes seven spheres of his sensory region, of which Luciani and Tamburini deal only with two, the sphere of the eyes and of the ears. The ocular sensory sphere, according to Munk, is found in front of the visual sphere, and corresponds in monkeys exactly with the angular gyrus (the visual centre of Ferrier), and in dogs with a zone widely extended transversely, which comprehends a large median segment of the first, second, and third external convolutions (including, therefore, a portion of the centre for the orbicularis palpebrarum, and of the visual centre of Ferrier).

When ablation of these regions is carefully performed, neither less nor more being removed, the following phenomena are said by Munk to occur. When the opposite eye is pricked or touched, the lids being held open, the animal merely winks, but does not otherwise move. When the eye of the same side is similarly handled, the animal attempts to protect itself and to get away, besides winking violently. The opposite eyelids do not wink on mere manual gesture before the eye without contact. In Munk's view, this implies simple paralysis of the sphincter, as according to him the visual perceptions and conceptions are intact. When the eye of the dog is bandaged, or a stitch put in the eyelids of the monkey on the same side as the lesion, the animals take by mouth or hand respectively small pieces of food offered them. Sometimes there is observed on the side opposite the lesion a slight ptosis and lacrimation, more or less, particularly in the monkey. No difference in the pupils. From these facts Munk infers that these are the special sensory spheres for the eyes.

In front of the auditory sphere, according to Munk, is found the special sensory sphere for the ears, corresponding in dogs with the upper portion of the Silvian or fourth external convolution; and in monkeys with the upper part of the superior temporo-sphenoidal convolution. In the first weeks after uni-

lateral extirpation of these regions he has noticed that the movements forward and backward of the outer ear of the opposite side are absent or weaker than those on the same side. In the dog there also occurs a loss of sensibility, particularly in the convex surface of the opposite pinna.

II. Of the effects of excitation and of destruction of the anterior or frontal portion of the second external convolution of the brain in dogs.

To Goltz belongs the honour of having shown that ablation of the motor zone in dogs is sometimes followed by blindness of the opposite eye. The authors observed this in only one case (Expt. XIX. of their previous communication), and in that case they think it not unlikely that the inflammatory softening, to which they trusted for destruction of the motor zone, had involved also the psychic centre of vision. They point out, however, that Ferrier's area 7.7.7. on the frontal portion of the second external convolution occupies an extent of surface quite disproportionate to the very small motor function which he localises there, namely, *conjoint action of the orbicularis oculi and zygomatics.*[1] In point of fact, they shew that removal of the anterior portion of the second external convolution in dogs is followed by disorder of vision on the opposite side, not amounting quite to blindness, but to a marked obscuring of the perception of visual images, so that objects appear as if covered by a mist. This obscurity passes off after a time. (Expts. II. and III.) And they state it as their opinion that the contraction of the opposite orbicularis oculi which follows irritation of this portion of the cortex should be considered as a reflex effect of subjective sensation, comparable with that which is commonly observed on the sudden appearance of a bright light before one eye. To the question: Where then is located the centre for movement of the orbicularis oculi, if the frontal portion of the second external convolution is partly a visual centre, as they maintain? they cannot give a direct answer. But inasmuch (1) as destruction of this zone is fol-

[1] Ferrier mentions movements of the eyes as also following irritation of this centre; but the authors' statement still holds good, that there seems to be set aside for these simple actions a very large portion of cerebral substance.—Tr.

lowed by ptosis of the opposite orbicularis oculi, and (2) because certain epileptic seizures, which have been proved to depend essentially on a state of tension of the motor zone of the cortex, begin in the orbicularis, they think the motor centre for the orbicularis is probably located in this portion of the cortex. And they think (though without offering proof) that this special centre probably lies well forward near the rest of the motor zone. The relation of the authors' view to that of Ferrier is now sufficiently evident. As to Munk, he places this portion of the cortex in the sensory region of the head, destruction of which would in his view cause psychic paralysis of the opposite half of the tongue and of the muscles of the mouth, and loss of sensibility in the opposite half of the face generally; but not one of his 100 experiments brings out the importance of this segment of the cortex to vision.

III. Of excitation of the median or parietal portion of the second external convolution in the brain of dogs and cats.

According to Ferrier, excitement of this part induces the directing of the eyes to the opposite side; and in the experiments of the authors (IV., V., VI., VII., VIII., IX.) there is substantially the same effect, though once (Expt. VII.) there was seen movement of the opposite eye to the *same* side. But this happened after an epileptic seizure.

They say, however, that they have localised more definitely than Ferrier the effects as regards the action of the orbicularis and palpebral elevator. He says that the effect of irritation of this region is contraction of the orbicularis, tending to close the eye; though if the eye was closed, to begin with, it opens slightly at the moment of passing the current. They say that contraction of the orbicularis (closure of the eye) occurs when they irritate the anterior, and opening of the eye (contraction of the elevator) when they irritate the posterior part of this region. Further they insist, in opposition to Ferrier, that myosis is far from constant; that sometimes it is absent, and sometimes there is midriasis.

They do not think these different results can be explained by different degrees of narcosis, of excitability, and of stimulus

applied, and they come to the conclusion that this part of the brain is not an organ functionally homogeneous. It is, they say, only on ablation of the centres that sound conclusions can be founded, and as Ferrier admits that there are but few exact experimental data to go upon, they have paid the more attention to

IV. The effects of destruction of the median or parietal portion of the second external convolution in dogs and cats.

Expts. X. to XIX. deal with this subject for dogs, and XX. and XXI. for cats, with the result that the parts of the brain in cats homonomous with the same parts in dogs appear to have the same functional value. The following general conclusions are drawn by the authors.

1. Unilateral destruction of the median or parietal portion of the second external convolution of dogs and cats produces immediately amaurosis nearly complete of the eye of the opposite side, and slight amblyopia of the eye of the same side.

2. These disorders are not permanent; but gradually pass off, until compensation apparently perfect is attained.

3. Bilateral destruction of this region produces immediately amaurosis almost complete of both eyes. This appears to pass off indeed, but not so quickly, so that after several weeks some traces of it still remain.

4. This disorder of the visual faculty on both sides is observed even when the bilateral mutilation takes place in two distinct operations, practised at a long interval of time and after compensation had been effected for the disorders consequent on the first operation.

5. These morbid phenomena of visual function take place with nearly the same intensity, when the destruction is limited exclusively to the middle portion of the second convolution, and does not affect parts of the third or first external convolutions.

6. They show on the other hand an intensity and duration notably greater, when the destruction, besides involving the median (parietal) portion, extends more or less also to the anterior (frontal) or posterior (occipital) portion of the second external convolution, and is not limited to the superficial

substance of the cortex, but extends more deeply into the grey matter surrounding the sulci.

V. Of the effects of destruction of the posterior or occipital portion of the second as well as the first external convolution of dogs.

According to the view of the authors it will have been seen that they consider with Ferrier that the median or parietal part of the second external convolution has the greatest importance so far as regards vision. But they assert in contradistinction to him that the centre is not confined to this region but extends forwards, comprehending almost the whole of the anterior part of the same convolution.

Munk, on the other hand, places the visual sphere in the extremity of the occipital lobe, its anterior boundary embracing scarcely any part of the visual region of Ferrier. He places the *sensory sphere for the eyes* in front of his visual sphere. He therefore makes two statements, a negative and a positive. On the one hand, he denies that the cortical visual centre of dogs extends much beyond the boundaries of the occipital lobe; on the other, he states that for the greater part of its extent the occipital lobe represents this centre. The first or negative part of Munk's thesis has been already dealt with by Luciani and Tamburini, who entirely disagree with it. And they take exception to the proof by which he supports it, pointing out that after extirpation of what he calls the sensory sphere of the eyes (in front of his visual sphere), the failure of winking which is noticed when gesticulations are made before the opposite eye, is due rather to interference with vision than, as he maintains, to the fact that the cerebral cortex is no longer competent to put the palpebral sphincter into action.

In order to settle the second part of Munk's thesis, our authors have performed Expts. XXII. to XXV., and have removed the posterior portions of the first and second external convolutions. In three of the four experiments the animals continued to see, they walked freely, could see a lighted taper and follow it with their eyes, and did not stumble against furniture or other obstacles, &c.

In Expt. XXIV. the upper and posterior part of the second external convolution was exposed on both sides in a young dog, to the extent of 3½ centimetres, by means of an oval opening. The whole region responded to electrisation, there being conjugate-deviation of the eyeballs to the side opposite the excitation.

They then destroyed by the sub-meningeal method not only the two portions laid bare, but also the greater part of the occipital lobes, dipping the scoop backwards against the tentorium, and inwards as far as the falx. Hæmorrhage rather copious. Notable consequent depression.

Next day the animal was somewhat depressed. On moving and shaking a lighted taper before the eyes, there was no reaction, or very little; but on threatening the animal with slight blows with the point of the foot, it responded by movements and cries, even when the foot did not reach the snout. The sensibility to pain most acute. Would not move.

During the following six days the animal behaved apathetically. Would not walk, and when compelled to, growled threateningly. Showed active signs of fear, and on being touched, howled lamentably, &c.

Here then was undoubtedly disorder of vision, but the authors believe that the removal of brain substance extended forwards, and involved without doubt a part of the excitable area, where Ferrier localises the visual centre.

While, however, the authors deny that this part of the brain is the visual centre, they do not deny that dogs in whom it has been removed behave differently from normal dogs. They are, for example, apathetic and indifferent to their surroundings; when not running after their food, they tend to remain crouched and inert, seldom responding by winking to gestures made before their eyes; they lose their natural affection, and, if threatened, they appear overcome with fear, and respond rather in defence than offence. This combination of phenomena agrees fairly with those described by Munk as the effect of destruction of the central portion of the visual sphere. But they cannot agree with his explanation when he says, "By the extirpation the dog becomes psychically blind (*seelenblind*), i.e. it has lost the visual representations (*Gesichtsvor-*

stellungen) which it had, or the mnemonic images (*Erinnerungsbilder*) of its former visual perceptions (*Gesichtswahrnemungen*), so that it no longer knows or recognises what it sees; but the dog sees; the visual sensations reach its consciousness and are noted, and allow of the formation of concepts regarding the existence, form, and locality of external objects, by means of which it anew acquires visual concepts, anew mnemonic images of visual perceptions."

The authors put their objection to this view in the form of the following dilemma. "Either the cortical centre in which visual perceptions have their seat is (as Munk assumes) also the centre in which mnemonic representations of previous perceptions are deposited, in which case the loss or oblivion (amnesia) of the representations implies as a consequence loss of the perceptions; or the seat of the mnemonic representations is distinct from the seat of the perceptions, in which case the latter and not the former will represent the true visual centre of the cortex."[1]

VI. Under the head of the effects of electrifying and destroying the parietal portion of the third external convolution in dogs, the authors formulate these conclusions.

1. Unilateral destruction of the upper and posterior part (parietal region) of the third external convolution in dogs produces immediately bilateral deafness, but to a greater degree in the ear of the opposite side.

2. The difference in the auditory sensibility in the two sides constantly diminishes, and after some days disappears altogether; though there are no sound arguments to prove perfect recovery of the auditory faculty.

3. After comparative equalisation of the power of hearing, removal of the corresponding region of the other hemisphere is followed at once by almost absolute deafness, which is approximately equal on both sides.

4. This bilateral cophosis passes off gradually, but data do

[1] I hope on a future occasion to make some reference to what as it stands does not seem to do more than raise the verbal question—What part of the cortex should be defined as the visual centre? Does it not also to some extent imply a misunderstanding of Munk's position?—A. R.

not yet exist for determining whether and when perfect compensation takes place.

5. When removal is confined to the second external convolution, and does not extend to any part of the third, there does not occur the slightest sign of dulness of the auditory sense, which indeed sometimes appears unusually sensitive.

VII. Of the effects of electrifying the angular gyrus in monkeys, as well as the surrounding portions of the cerebral cortex.

The widest divergence of view between Ferrier and Munk appears under the question of the localisation of the visual centre in the cortex of monkeys. Ferrier places it in the angular gyrus or *pli courbe*, while Munk locates it on the convexity of the entire occipital lobe. Ferrier's view is thus expressed: On electrifying the angular gyrus, "The eyes move towards the opposite side, with an *upward* or *downward* deviation, according as the electrodes are on 13 or 13′" (anterior or posterior limb of the *pli courbe*). "The pupils also generally become contracted, and there is a tendency to closure of the eyelids, as if under the stimulus of a strong light. The head frequently follows the direction of the ocular movements, though this is not always observed."

When the superior temporo-sphenoidal convolution is stimulated, there is "pricking of the opposite ear; head and eyes turn to the opposite side; pupils dilate widely."

And according to him the whole cortex of the occipital lobe as also the middle and inferior temporo-sphenoidal lobes are absolutely inexcitable.

The conclusions of the authors are thus expressed:

1. The only one of the phenomena noted by Ferrier as occurring on stimulation of the angular gyrus, which always does occur, is conjugate deviation of the eyes to the side opposite to the one excited. The contemporaneous elevation or depression of the eyes does not constantly follow the rule indicated by Ferrier.

2. The contraction of the pupils noted by Ferrier, although it sometimes appears, is by no means a constant occurrence. Sometimes the pupils undergo no change; but oftener there is midriasis more or less marked.

3. The closure of the eyelids is also a phenomenon of inconstant occurrence, which is not uncommonly replaced by opening of the lids.

4. The occipital lobe, and the upper portion of the middle temporo-sphenoidal, which Ferrier declares to be quite inexcitable by the electric stimulus, appear, on the contrary, in our experiments, capable of reactions perfectly similar to those which arise on excitation of the angular gyrus, only less conspicuous. This is the most important difference between the assertions of Ferrier and the results obtained in our experience.

VIII. Of the effects of decortication of the angular gyrus, as well as the occipital lobe, and of the superior temporo-sphenoidal lobe in monkeys.

Ferrier's view is given in these words: "Destruction of the angular gyrus on one side causes blindness in the opposite eye. The loss of vision is complete, but is not permanent if the angular gyrus of the opposite hemisphere remains intact; compensation rapidly taking place, so that vision is again possible with either eye as before. On destruction of the angular gyrus in both hemispheres, however, the loss of vision is complete and permanent, so long at least as it is possible to maintain the animal under observation."

This opinion is not endorsed by the authors who detail Expts. XLV. to LII., which they performed in order to test the question. Their conclusions do not quite agree either with those of Ferrier or those of Munk, and they admit that their conclusions are not so satisfactory in the case of monkeys as in those of dogs. First, they have never seen blindness follow removal of the angular gyrus on one or both sides. This is totally opposed to Ferrier. Next, they think that removal of the angular gyrus (with or without a small portion of the occipital lobe—Expts. L. and LII.) is followed by hemiopia, or relative blindness of the half of the retinæ corresponding with the side operated on. Munk first observed the phenomenon of hemiopia, but the authors are at issue with him to this extent, that Munk assumes that it is only destruction of the occipital lobes which can induce disturbance of

vision (amnesia of visual images, according to him), while they attribute it to destruction of the angular gyrus. They therefore conclude that the visual centre in monkeys is not confined to the occipital lobes, but embraces also the angular gyri.

As regards the auditory centre, they have no decided views to advance. They, however, think it probable that, like the visual centre, it acts bilaterally by semi-decussation of the auditory fibres, and they lay some stress on the fact that the temporo-sphenoidal lobes are anatomically correspondent in monkeys with the third external convolution in dogs, which they have determined to be the seat of the auditory faculty in these animals.

The authors recapitulate their conclusions as follows :—

1. The visual centre in dogs is represented by a long zone of the cortex of the second external convolution, which extends from about the frontal portion to the occipital lobe. That of monkeys probably comprehends not only all the angular gyrus, but also a great part, if not the whole, of the convexity of the contiguous occipital lobe.

2. The auditory centre in dogs is certainly represented by the upper and posterior part of the third external convolution; but it is not certain that it may not extend beyond these limits. In monkeys this centre is presumably contained in the region homologous with that of dogs, i.e. in a zone just external to the visual centre represented by the middle and superior temporo-sphenoidal convolutions.

3. The visual and auditory centres, both in dogs and monkeys, are electrically excitable in the whole of their extent, though the reactions obtained at different points vary not only in degree, but often even in character.

4. These reactions may possibly be due to reflex movements consequent on visual or auditory sensations; but it is much more likely that they depend on direct excitement of the special motor centres of the muscles affected, centres probably included in the visual and auditory regions.

5. Unilateral destruction of the visual zone in dogs induces at once almost complete amaurosis in the eye of the opposite side, and slight amblyopia of the eye of the same side. In

monkeys, on the other hand, unilateral destruction is followed by bilateral hemiopia of the half of the retinal field on the side operated on. It appears therefore likely that almost complete crossing obtains in the fibres of the optic nerves of dogs, and semi-decussation in those of monkeys, wherever or however the semi-decussation may occur.

6. Munk's view of psychic blindness (amnesia of visual representations) is not tenable, since the blindness produced by ablation is not only psychic, but implies also affection of visual perceptions. No change observable by the ophthalmoscope in the fundus of the injured eye.

7. Bilateral destruction of the visual zone in dogs, if very extensive, produces at once almost complete and bilateral amaurosis. In monkeys, incomplete bilateral destruction of the visual zone produces simple bilateral amblyopia.

8. Analogous effects are produced by similar treatment of the auditory centres in dogs.

9. All the effects, whether of uni- or bilateral destruction (both in dogs and monkeys) pass off in time, their duration being proportional to the intensity of the phenomena and the amount of destruction of the centre. It is not certain whether perfect compensation occurs.

10. Compensation for the effects of unilateral destruction of auditory and visual centres is effected, at least partly, by increased action of the opposite side. For, on removal of the second half of the centres, blindness and deafness occur on both sides.

11. Compensation after incomplete bilateral mutilation is effected by those parts of the centres which remain intact. Should future investigation show recovery after complete bilateral mutilation, it would be necessary to suppose that it is effected by the basal ganglia, in accordance with the authors' views regarding recovery after complete ablation of the psychomotor centres.

To settle this last question the authors performed Expt. LIII., in which they succeeded in removing both angular gyri and occipital lobes in a monkey. Vision was partly retained, and partly rapidly reacquired. From which they infer the justice of their former opinion that the basal ganglia take on

an increase of their functional value and become centres of vision.

A. Rabagliati.

The Colour Sense: its Origin and Development. By Grant Allen, B.A. London, Trübner and Co. 1879.

This work is the tenth volume of the "English and Foreign Philosophical Library," and, though it differs much from its predecessors in the nature of its subject matter, being devoted to Applied, instead of Pure, Philosophy, yet the importance of the doctrine of evolution to more abstract speculation fully entitles a book like the present, which forms a highly interesting chapter in the history of the development of living beings, to a place in the series among which it has appeared.

As mere matters of fact, the contents cannot perhaps lay claim to much absolute freshness, for they are mostly drawn from well-known recent works on natural history; but there is undoubtedly much genuine originality in the way in which the facts are strung together; so that, even though the laws and modes in which evolution has occurred in the organic kingdom may be already familiar, yet they are here shown, in the department now treated of, to have an extent of application that would scarcely have been expected: the chief feature of the book being that the factors are described not so much separately, as has hitherto usually been done, but in their mutual dependence and interaction, the individual links being shown really to belong to a long and compact chain; the result of the whole being an agreeable, clearly-drawn picture of the series of events by which we obtained our principal sense.

But the greatest charm lies not solely, and perhaps not chiefly, in the leading argument itself, but in the general clearness of exposition of the details, and the artistic way in which the description is made, as well as in the abundant seasoning with keen inductive evidence, from which the author seeks to obtain the proofs of the several stages of his argument.

The author seems to aim at throwing the brilliant rays of the sun upon a region hitherto lighted only by diffused reflected beams; and, although he has occasionally to displace

another luminary, the change is one almost from darkness to brilliance.

To give some notion of the aim, scope, and method of the work, we will try to pick up the thread of the argument by the end, and unravel it continuously, step by step.

Starting from the admitted fact that many animals, besides man, not only can discriminate between varieties of colour, but also derive enjoyment from the sensation of bright hues, the author lays himself out to solve this twofold problem—why animals are sensitive to colour at all; and why certain sensations of colour should yield pleasure, while others do not? The solution is sought by first establishing empirically and inductively certain apparent facts of natural history; and then showing how the observed relations are explicable by, and fully accord with what might have been deduced from, well-known physiological principles. The problem opens thus: to find what is essential to any particular source of pleasure, it is needful to know what is pleasure in general; it is defined as "the psychical aspect of an ultimate physiological fact, . . . the unimpeded activity of a fully-nurtured and not overworked organ, in structural connection with the cerebro-spinal or other sentient central nervous system." How, then, could such a structure, with colour perception as its function, come into existence? Organic evolution is of course taken for granted. Essential to such a process is the repeated stimulation by some object which causes benefit to the individual, for "no faculty can be originally developed for mere useless exercise in unessential acts." We must therefore find out what objects exist in nature, giving forth bright colour, and also affording advantage to animals. On making the necessary survey we are met by the striking fact that such conditions are fulfilled by only a few substances—viz. some members of the organic world—to wit, certain flowers, fruits, and animals: for though the vault of the sky, the rainbow, and most gems present pure and brilliant tints, yet the former are too common, the latter too rare, and all too indifferent in value, to produce any impression on animals in whom a highly developed æsthetic faculty does not already exist. Even the green vegetable foliage would be too uniform and too indiffe-

rent to call for regard. At one time (e.g. in the Carboniferous period) there was probably no colour in the vegetable kingdom besides a nearly unvaried green, and probably no colour sense in animals either. How, then, could bright colours be brought into existence? Recently acquired knowledge—especially Darwin's and Lubbock's observations—have thrown much light on these processes; and the author undertakes to prove that bright colour has arisen in plants simply to attract the notice of animals; and that the sense organ to receive such impressions has developed in animals in correspondence with the increasing stimulation. The object of plants in seeking the attention of animals is to procure aid in cross-fertilisation. The object of animals in seeking plants is to procure food. At first, all plants are self-fertilised, but this produces less vigorous individuals than cross-fertilisation; if, therefore, the latter mode were to replace the first, in any species, natural selection would favour the new at the expense of the old type. The advantages of cross-fertilisation are so great that almost any way of effecting it would be attended with benefit. Two expedients have offered themselves, and been adopted in different cases, to procure the necessary dispersion of pollen—(*a*) the wind, (*b*) insects. These three modes of fertilisation correspond fairly with three palæontological periods, which may therefore be called those of— (1) self-fertilised, (2) anemophilous, (3) entomophilous plants. Of the two latter, the last is by far the best, being surer, and less wasteful: the plants in which it takes place will get an immense start over the rest in the race for life; and thus an excellent opportunity would be afforded for the operation of natural selection, if only the necessary variations could be brought about. There has probably been not much difficulty in this respect. Insects had probably been already in the habit of visiting plants, to obtain food in shape of pollen. Now, during the process of reproduction, chemical changes take place in plants, oxidation alone occurs instead of reduction, with the formation of sugar, other colouring matters in place of chlorophyll, &c. This colouring matter contrasts with the green of the surrounding leaves, and serves as a beacon to the incipient colour sense of the visiting insects, in whom the sense now goes on to improve, under the influence

of habit and natural selection. Any increase of sugar, also, will act as an extra inducement to insects to pay visits to the plants producing it, as well as being an economy in pollen to the plants; and thus an extra, otherwise useless, supply comes to be often formed, and these plants will be indicated to insects by peculiar or extra bright colour. But colour is not the only means of attracting animals that is resorted to. The incipient sense of smell is also appealed to, and the perfume and corresponding organ of smell proceed to develope by gradual variation and natural selection, just as colour and its sense organ have been shown to do. So great does the author believe to be the resources possessed by plants, that he states in general terms that any part of a plant that will be benefited by the notice of animals, will forthwith tend to develope in it bright colour and sweet smell. Though the bright colours are at first produced in all parts of the plant indiscriminately, they gradually become chiefly concentrated in certain parts—usually a few leaves, which become the petals. The primitive flowers were regular and polypetalous; but became irregular and gamopetalous in becoming more adapted to the insects visiting them, in order that the end aimed at shall be more readily brought about. In this way, the author believes the senses of sight for colour, smell, and taste to have arisen. The same device has arisen separately in monocotyledons and dicotyledons, and has probably originated afresh innumerable times.

Something similar has taken place in the case of fruits. The period of maturation of seed is the second critical epoch in the life of a plant, in which its fortune depends much on circumstances. As a large proportion of all mature seeds perish from lack of nourishment for the embryo, any means of ensuring a supply for the new plant would be attended with advantage to the species. It has been more or less completely effected or replaced in three chief ways: 1. Replacing the seed by an underground stem with a supply of nutriment and tendency to germinate. 2. Supplying a store of nutriment to the germ for its use when activity commences. 3. Distribution of the ripe seeds over a wider area, so that the chances of getting sufficient nutriment in the soil would be increased. But, in the first and second cases the extra supply of food

tempts the appetite of animals; for the store is equally good food for both, and is a richer and more economical kind than animals could otherwise obtain; this method would, after all, often be injurious to the vegetable species, unless some means of protection against these depredations be acquired. This in many cases has been done—in the former mode by the store remaining underground, out of sight; in the latter, the seeds having acquired a hard coat, to defy the teeth, a dull colour to escape the notice, and often a bitter outer husk to displease the taste of the animal who would eat them: this is the mode of the genesis of the nut. But in the third case, dispersion may be effected either by the wind—in accordance with which many changes for facility in conveyance have come to pass in seeds; or by the aid of animals—drawn to the seed in search of food; but, though animals will pay no visits unless they are rewarded by food, yet no advantage will accrue to the plant if the embryo itself be destroyed. It is therefore essential to allure and attract animals, and yet protect the germ. The accomplishment of these ends results in the formation of a fruit (using the term in its popular sense). The actual process is much like that of the origin of flowers—out of the same tendencies to variation as were previously described, and which occur at this period also, result the formation of sugar, bright colouring matter, and perfume, to appeal to the senses of taste, sight, and smell of animals, in this case, mostly birds and mammals. But in them, fruits do not work so complete a change as flowers do in insects; for the vertebrata have already probably some sense for colour, inherited from some common marine invertebrate ancestor, in whom the colour sense had probably been gradually awakened in a precisely similar manner, by the attraction of bright coloured food. The colour sense being thus produced, pleasure results from its exercise, according to the law enunciated above. The author is careful to make this statement, to prevent his being understood to mean that the pleasure of bright colour was merely extrinsic —due to the transference of the pleasure of gratified appetite and successful search, produced by the finding and eating of fruit, to the visual sensations by which such success has been attended, according to the law of association.

When the mere exercise of the sense has thus become pleasurable, it is exerted in other ways: notably in sexual selection; by which process, the continual preferment of bright-coloured mates by the females has led to the establishment of bright-hued species; as in plants, an original tendency to produce colouring-matter existed—especially in the tegument: many waste matters of the organism being pigmented and attracted to and fixed in the skin as a vicarious excretory organ.

But colour, except in a few cases, does not attain a higher degree of intrinsic beauty in animals below man; in him, however, the love of colour for itself leads to the decoration of his person, house and property with bright colour; and lastly, to the appreciation of beauty in the gorgeous hues of the sky at sunset.

Such is a brief and imperfect outline of Mr. Allen's theory; the chief points on which it hangs are the soundness of his physiological theory of pleasure—which seems to be a fair statement of the truth, although he perhaps does scarcely sufficient justice to its rival—the association theory of pleasure; for there is plenty of room for both. But its main support, to which the chief part of the book is devoted, is the accuracy of correspondence between possession of colour sense and habits of eating coloured objects. It is in discussion of these matters that the author displays his best reasonings and skill for investigation—shown by devising passive experiments; and he gives many examples of the different inductive methods. Although he certainly traces a most accurate correlation in numerous crucial cases, yet he acknowledges the occasional apparent impossibility of explanation of others, and even rarely a seeming opposition. However, it is very likely that an extensive acquaintance with natural history might clear away these obstacles; and at any rate this would be the best way of obtaining a decision.

The positive part of his theory being established, Mr. Allen proceeds—and with success—to combat the rival scheme of Gladstone, Magnus, and Geiger: who hold that the linguistic evidence of the Vedas and Zend Avesta, the Bible and Homer, shows that in early historical times man had little or no

appreciation for differences in colour; but that, since then, he has gradually acquired a perception for red, yellow, green, in order, and lastly blue.

But by reference to numerous existing savage races, as well as to still earlier historical and even prehistoric peoples, he shows almost with certainty that they really had a well-developed taste for variety of colours; though their speech affords insufficient evidence.

The final chapter contains a description of the mode of growth of the colour vocabulary; but is not much more than a particular example of the process of word-growth already described by Adam Smith and his successors.

Though probably all will admit the author's explanation of the important bearing of coloured food in developing the colour sense, yet all his readers may not feel satisfied that it has been the sole mode. For, though it might account for the greater degree of pleasure derivable from red, yellow, and blue, than from green, yet, according to his physiological data, we should have expected that the pleasure derivable from a sense would be proportional to its degree of development, and therefore to its keenness of discrimination, i. e. of discerning fine relations and minute differences; yet this does not appear to be greater for bright colours than for others. Also, it would be very difficult to explain, by this theory alone, the converse facts of distaste. Also it would still leave some other senses—especially that of hearing—still unexplained,—and almost hopelessly so. But in the latter case a large proportion of the sensation of harmony seems to result from physical harmony of sounds; before dismissing altogether, therefore, the opinion that a similar condition—of harmony —might be the physiological conditions of pleasurable colour, it would perhaps be advisable to wait until the physical basis of vision has become better known to us; and perhaps nothing would lead more to this than a thorough comparison of the physiology of our two chief senses.

F. L. Benham.

Clinical Cases.

A CASE OF DOUBLE OPTIC NEURITIS, WITHOUT GROSS CEREBRAL LESION, WITH REMARKS UPON THE IMMEDIATE CAUSATION OF OPTIC NEURITIS. (*Illustrated.*)

BY STEPHEN MACKENZIE, M.D.,

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The following case is of such interest that I hope I shall be pardoned for the detail with which I have narrated it. The patient had evidently been for a long time on the border-land of insanity, and the acute maniacal delirium which terminated the case may be taken to indicate that she had passed beyond the border. As in so many cases in which the mental faculties are at fault, no coarse anatomical changes were found in the brain beyond wasting of the organ. Microscopic examination, however, showed changes which fully accounted for the symptoms presented. The long period over which the mental symptoms extended would point to the brain being primarily at fault. Atrophy of the brain such as she had is sometimes found in arterio-capillary fibrosis. No such condition was, however, present in her case. Her body was well nourished, the kidneys healthy, the left ventricle not hypertrophied. In cases of atrophied brain and cord in connection with arterio-capillary fibrosis, similar exudative changes have been found by Sir William Gull and Dr. Sutton, and it is noteworthy that the end of such cases is frequently ushered in with acute symptoms referable to the brain and spinal cord. Indeed this seems to be in accordance with a broad pathological law that tissues in a state of decay or of impaired nutritive activity are prone to become inflamed.

The symptoms presented at one time resembled hysteria, and the attacks were thought by some to be hysterical. In this respect the case recalls the valuable one recorded by Dr. A. Hughes Bennett, in the first number of 'Brain.' In his case there was a tumour; in the present there were marked,

though minute, structural changes in the brain. In many of the attacks the patient had spasm of the pharyngeal and laryngeal muscles, reminding one of hydrophobia. The nurses of the ward, who have seen three or four cases of hydrophobia in the hospital, were struck by the resemblance. The case is as follows:

Marie Schwabe, a Frenchwoman, a native of Paris, aged 27. When I saw her, in September 1878, she was a woman of somewhat short stature, inclined to be stout; dark hair, almost black, and with long hairs growing from upper lip and chin; teeth good. Her expression was that of a woman who had suffered a great deal—an expression of anxiety and despair. Patient's father and mother living; the former had been afflicted with hemiplegia for some years. She has two sisters and one brother, living and healthy; no history of phthisis, epilepsy, or insanity in family. Her previous history was obtained chiefly from her husband, an intelligent clerk.

She had good health and manifested no mental peculiarity, as far as is known, up to the time of her marriage, at 18. During the whole of her married life, however, she used constantly to express to her husband a dread that she would go mad; and "she used to dream almost every night of mountains, serpents, lions, and other animals, with the heads of people she knew." She used also to complain of a sense of suffocation if she was in a room with the door shut. Her memory was impaired for ordinary affairs, but was unnaturally acute in recalling anything bad of those she knew. Was not pregnant until three years after her marriage. When five months advanced in pregnancy she had a very bad miscarriage, without assignable cause. She had been complaining of obscure pains over the region of the uterus for some fourteen days previously. She was attended by a doctor, but lost a good deal of blood. The following day, on her husband coming home in the afternoon, she complained that the nurse whom he had engaged to look after her had been drinking brandy, and was drunk. (This was quite imaginary.) She then, in a wild manner, said several times, "You give her coffee." She became extremely excited, sprang up in bed, tore down her clothes, which were hanging on a nail by her bedside, and said, "I want to go away—I want to go away." She then had a kind of fit, similar to those she had subsequently in the hospital—repeatedly opening and shutting her mouth, stoppage of respiration, turning pale, violent struggling, and "became insensible." The attack lasted about three or four minutes: she did not bite her tongue. She gradually recovered her senses, and remembered nothing of what had occurred. A few hours after this she had a laughing fit, and repeatedly said to her husband, "You

laugh with me!" Next day she had "convulsions" from time to time, and a peculiar twitching of the muscles of her face. This twitching lasted continuously for one week, and since that time to her death she had this twitching, if excited. She improved a good deal for three weeks after this, when she again became nervous; and one day, on her husband opening the door, said she believed that the members of the club to which he belonged would poison her, and expressed a great dread of everybody she knew.

Menstruation had been irregular since her miscarriage; she had excessive flow every two or three weeks. After her miscarriage she walked with difficulty, and vomited after her food. She consulted a doctor, who placed a pessary in position, with temporary relief. She went into the country for a few weeks, but at the end of that time became nervous again, "lost the use of her limbs," and suffered constantly from severe headache. She says sometimes the pain was so severe she could not think; she used to say stupid things which she afterwards remembered. About nine months before her death she began to take a great dislike to people whom she saw for the first time, avoided persons her husband brought home to supper, and complained to him that he forced their society upon her. She took to eating the tops of lucifer matches (a favourite continental method of committing suicide), and her husband had to remove them forcibly from her. The least noise, or any one speaking to her, made her very nervous. She says that now she is not able to control her feelings, and burst out crying if refused anything she asks for, and this distresses her a good deal. For three months before admission took narcotics at bedtime, but even then has not had proper sleep, always being restless, and having disturbing dreams, seeing strange figures, &c., which have caused her to wake suddenly, and become very agitated. In one of her dreams she thought she was eating her own brain. For three days previous to admission she had suffered so much from headache that she could not be left alone, and she vomited after everything she took. The headache had been far greater than previously the six weeks preceding admission. Throughout her illness she was troubled a good deal by constipation.

The patient had had an operation performed on her right eye (for strabismus?) in childhood. For six months preceding admission she had been complaining of weakness of her eyes, especially the right, but ever since her husband has known her, she has had to put her hand over her right eye on reading or writing. There is no evidence to show that her left eye had been failing previous to admission, beyond the fact that on the day of admission she had been previously to see some

friends, and on entering the room had complained that she could not see any one.

A week after her miscarriage a Hodge's pessary was introduced, and she wore it for some months until it slipped out during defæcation. She then had it replaced, and wore it until the end of last March, when she was told the womb was in place. A few weeks later, when straining at stool, she felt the womb fall. The pessary was again introduced, and she wore it until a few days before admission, when she took it out herself.

She applied at the obstetric department of the London Hospital, on account of her uterine symptoms and of increasing pain in the head, and was admitted under the care of Dr. Herman, August 28th, 1878.

She then complained of great pain in left ovarian region, and down left leg and side, pain in the head, and dizziness. On vaginal examination, the uterus was found to be retroflexed, movable, and the fundus large and exceedingly tender. There was an erosion on each lip of the cervix, with a copious glairy discharge from cervical canal; the cervix was blue and congested. The cervix was scarified two or three times, and a mixture prescribed consisting of sulphate of magnesia and bromide of potassium. The uterine congestion having subsided, a Hodge's pessary was fixed, and the womb found to be well-retained in position.

Her other symptoms, however, increased; the headache was very severe, and she frequently vomited—sometimes clear fluid, sometimes green fluid. I was therefore asked to see her on Sept. 22nd.

I found her exceedingly nervous, starting whenever any one approached her, and looking extremely frightened. She complained very much of her head, and implored relief. Physical examination of her chest and abdomen revealed no important changes. Her tongue was coated with dark brown fur; bowels much constipated. There was no œdema of feet.

Ophthalmoscopic examination of left eye showed swelling of the optic nerve, the margin of the disc being wholly lost to sight. Owing to patient's restlessness, no detailed examination could be made. The right eye could not be examined with the ophthalmoscope, owing to old iritic adhesions.

Her vision was not tested by types, but the following facts indicate its condition. On her admission (about a month before I saw her, and before the neuritis was discovered) she wrote to her husband: this letter was a long one, and written in a small ordinary hand. Her letters gradually became shorter, and the type larger and larger. In three weeks she could not see to write at all, and got the patient in the next

bed to write for her; from time to time she asked her husband to write to her in larger type, and complained even then. She had to hold his letters quite close to her eyes. She could see quite well previous to admission: she could use her right eye for near objects (to read), but not for distant objects.

Her temperature since the beginning of September has averaged nearly 100° Fahr., the maximum being 101°, the minimum 98·4°: the evening temperature being usually higher than the morning. Urine, s. g. 1026. Urea 3·3 per cent. A very faint trace of albumen, a little epithelium only in deposit.

She was transferred to my care.

Sept. 30th, 12.30.—She screamed out, was purple in the face, and tried to tear her pocket-handkerchief. When seen by the medical officer, she was quite quiet, but not unconscious. When spoken to, answered questions quite rationally. She asked to have her husband sent for. At times she cried out; at others made a peculiar yelping noise. When I came to her bedside in the afternoon, she was lying in bed quite quietly, with a very anxious expression of countenance. She said she felt comfortable, but was very anxious that something should be done to relieve her head. Whilst talking to her she was seized with spasm of the laryngeal and pharyngeal muscles; her mouth would be suddenly opened two or three times in succession; she made a gasping noise, and occasionally a sudden inspiration, accompanied by spasm of the larynx, which produced a yelping or barking noise. The spasms appeared occasionally like exaggerated hiccough. Some of the noises she made occurred during expiration. She said "Give me some water," and when a glass of water was placed in her hands, she could drink, swallowing without any great difficulty, but her agitation and tremor made it difficult for her to hold the glass to her lips. She seemed very anxious about herself, and said inquiringly, "I am not dying?" She frequently asked for water, and each time drank some. The spasms continued for about a quarter of an hour, as long as these notes were being made. She complained of a burning sensation, "like a fire," in her chest. She occasionally put her handkerchief into her mouth and bit it.

The following mixture was ordered to be taken every 3 hours:

℞ Potassii Bromidi
Chloralis Hydratis ā gr. x.
Acid. Hydrocyanic. dil. ♏. iij.
Aquam ad ℥j.

8.30 P.M.—She complains during intervals of quiet that the bowels have not moved; she still has burning pain over

epigastrium, and severe headache. Her teeth are chattering (temp. 99·7°). She keeps twisting her pocket-handkerchief between her fingers. She has had a little quiet sleep since taking her medicine, but on waking up the spasm of the pharyngeal and laryngeal muscles returned. She can talk quite sensibly during the intervals. On giving her some water she drank it readily, but spasm followed immediately. She caught at the bedclothes, and made a peculiar rasping or barking noise, and then threw herself back in bed. She says the cause of her attack was seeing a big woman with a shawl over her head, walking up and down in front of her bed last evening, and that the woman struck her head with something black. She has not vomited to-day. Pulse 96, small.

2 A.M.—House Physician called to her bedside. He found her yelping, and on giving her something to drink, spasm immediately followed. After an interval of quiet, she began laughing, and continued to do so for about five minutes. She said, "You don't laugh; you see I do!" This was followed by a fit of crying.

Oct. 1*st.*—She has had about four hours of quiet sleep in all, but each time on waking up immediately began to make a yelping and barking noise. During intervals of quiet she protrudes her tongue when asked to do so. She says the pain in her head is still very bad, and she adds, "but I have more reason this morning." She complains of something which she says is grasping her throat with two hands, and also of severe burning pain over epigastrium.

9 P.M.—After $\frac{1}{4}$ grain of morphia hypodermically, patient was sleeping quietly, and afterwards passed a good night.

Oct. 2*nd.*—On coming to her bed this morning she was quiet, but immediately after being spoken to began yelping. This continued for about five minutes, and was followed by a fit of laughing, during which she threw herself about in bed. She appears afraid lest she should be going to die, and continually asks if she shall get better. She has not vomited since yesterday; bowels were open last night after an enema. Passes her urine freely. The mixture was stopped until this morning. She still complains of severe headache. Is able to drink without any spasm being produced, but a short time afterwards made the same noises as previously.

Oct. 3*rd.*—Slept until one o'clock this morning, when on waking, began laughing and crying. On her husband persuading her to try and sleep, she became composed, and passed a good night. Has had no more vomiting. Drank a pint of milk, with only a little spasm. A hypodermic injection of morphia, gr. $\frac{1}{4}$, was given at 11 A.M. This morning, on coming

to her bedside, she immediately had a fit of laughing, during which she beat the bed with both hands. This was followed by shrieking out at the top of her voice, and then she said, "Boo, boo." Still complains of headache.

Oct. 4th.—Passed a quiet night; has not vomited, and says headache is better. She was lying quite quiet, when, on another patient in the ward laughing, she immediately broke out laughing, screaming at the top of her voice, beating the bed with both hands. Has taken some milk. Passed her urine this morning.

When I saw her in the afternoon, I dictated, "patient was always pale, but now her face is white; there are dark halos under her orbits. The left eyelids are swollen and red. Altogether she looks extremely haggard, and as though she had had but little sleep for some time. She is still very much excited, tosses herself about in bed, and frequently strikes the bed with both hands a number of times in succession, and with great force. Her tongue is coated; there is slight sordes on her lips. The bowels have not acted for three or four days. Left pupil contracted, and an ophthalmoscopic examination impracticable."

Oct. 5th.—Had an enema last night, after which her bowels acted twice. Had a quiet night until 4 A.M. The pessary she was wearing was removed last night, since then she says she has been easier. On coming to her bed this morning she immediately began to shriek out, to beat the bed with her hands, and then to throw herself back in bed. It is very difficult to keep her in bed, and she struggles violently when any attempts are made to restrain her. Has passed water. She keeps crying out about "the naughty woman," and says "she was very naughty to make me so ill." This was followed by a fit of laughing. Has not vomited.

Evening: was quiet on coming to her bedside, but immediately began laughing immoderately.

Oct. 6th.—Passed a quiet night after an injection of morphia, but is in much the same condition this morning; first laughing, and then crying. She requests every one who comes near her to laugh, and then does so herself; she afterwards bursts out screaming as loud as she can, becomes very violent, and at times it is a difficult matter to keep her in bed. No vomiting. It became necessary to place her in a separate ward, as her incessant noise disturbed the other patients She passed a large quantity of urine yesterday afternoon; has taken a fair amount of nourishment.

Oct. 7th.—Slept quietly until 2 A.M., when she became very noisy, laughing and crying. She is very irritable. Passed her urine twice yesterday, and her bowels have been freely opened.

Oct. 8*th.*—Passed a quiet night: no pain in head this morning. Has passed her urine; bowels open in the night. She has been much quieter this morning, but called her husband "a fish." She has not screamed so much this morning. Says she is losing her reason. No vomiting; takes nourishment very well.

Oct. 9*th.*—Passed a quiet night. Is more tranquil this morning. Has a haggard, worn look. Has not vomited. Has taken a basin of arrowroot this morning. Both upper eyelids are now swollen. Drinks without any spasm being produced. Tongue coated. Passes urine very freely, but in varying positions.

Oct. 11*th.*—Passed a quiet night. No albumen in urine: no vomiting. Says she has no pain in the head. Tongue thickly coated. Passes urine freely. Her expression is still more haggard.

Oct. 14*th.*—Passed a very restless night, in spite of morphia and bromide of potassium. Bowels confined: passes urine freely. Eyelids less œdematous. Complains less of pain in head. Says she is better. She does not scream this morning, but makes a groaning noise. Will not protrude her tongue when asked to do so. Takes nourishment.

Oct. 16*th.*—Passed a restless night. Her husband (who is almost constantly with her) says she is able to swallow, but after having done so makes a gurgling noise, and then anything she has taken returns. She has a worn-out look. Her pulse is small, 96. She does not appear to have strength to scream. Has passed her urine and motion in the bed. No paralysis of limbs. Does not complain of pain in the head, but still complains of pain over epigastrium. Recognises her husband and the nurse.

Oct. 17*th.*—Passed a quiet night. Has taken a fair amount of nourishment. When asked if she is better, says "Yes." She looks exceedingly ill; her pulse is small; feet and hands cold. She is very restless. Passes both urine and fæces in bed. Has been screaming more this morning.

Oct. 18*th.*—Passed a restless night. Has been screaming all night. Bowels moved freely yesterday. Has taken a fair amount of nourishment.

Oct. 19*th.*—Passed a quiet night, except at intervals. She is restless in bed this morning. On being asked if she has any pain, she does not speak, but shakes her head. Passes her urine in the bed. Takes nourishment.

Oct. 21*st.*—The patient passed a fairly quiet night. Nurse reports that several times, without any cause, her breathing stopped, and she turned purple in the face. After a few seconds there was a gurgling in the throat, and breathing

recommenced by her taking a long inspiration. This was observed several times (Cheyne-Stokes respiration). She does not answer when spoken to. Passes her urine in bed: bowels not open for several days. No paralysis of any of her limbs. Pulse 84, small and feeble. She cannot be roused. Swallows with difficulty. Feet and hands and surface of body cold. She is getting rapidly worse.

2 P.M.—Patient evidently sinking.

Died early in evening.

Her temperature averaged about 100° Fahr., the maximum being 101·6°, from Sept. 22 to Oct. 6. After this date she was too restless to admit of it being taken. On the two occasions her urine was examined, its specific gravity was 1026 and 1025 respectively, and contained 3·3 and 3·6 per cent. of urea. The quantity of urine secreted could not be measured. On the first occasion a very faint trace of albumen was present, on the second none.

The treatment consisted of large doses of potassium bromide, with and without chloral, of subcutaneous injections of morphia, of aperients as required, and the frequent administration of easily digested food.

The *necropsy* was made on the 22nd, at 3.30 P.M., by Dr. Sutton, in my presence. The following is Dr. Sutton's report, extracted from the Post-mortem Book:—

"Atrophied brain. Fatty deposition on right ventricle of heart. Much subcutaneous fat.

"Largely developed body, with large regular features. Limbs seemingly somewhat above the average bulk. No œdema. The subcutaneous fat over thorax and abdomen was about one inch thick.

"Skull and dura mater normal. Pia mater milky-looking and semi-opaque. The brain convolutions were much atrophied over anterior two-thirds of the hemispheres. The sulci were much widened. The grey matter of brain on section looks normal, merely venously congested, and the brain substance on section looked normal throughout. The vessels at the base were seemingly normal.

"The heart weighed 8½ oz.: it looked smaller than normal. There was much fat on the right ventricle near the apex: it had grown into the wall of the right ventricle, leaving scarcely any visible muscle: muscle more natural above. The left side of heart was natural, excepting that the wall looked thin.

"Lungs œdematous, and hypostatic pneumonia at back part of one.

"Liver and spleen normal. Kidneys normal. The mucous membrane of stomach was studded with a number of yellowish

masses, the size of pins' heads; they looked like stomach tubes distended with fat."

Dr. Sutton appends the following note to the case:—

"There was no evidence to show that the atrophy in this case was part of a general atrophic process. The post-mortem examination would leave it uncertain as to whether the fatty accumulation and degeneration occurred antecedently, rendering the right ventricle unable to empty itself properly—leading to venous distension of the pia mater and venules of the convolutions, and by pressure of these distended veins, or otherwise, producing the atrophy of convolutions, more or less dementia, and the subsequent obesity."

Microscopic Examination of optic nerves and retina, and of brain.—The right optic nerve was removed for the purpose of examination. This eye had not been seen during life owing to iritic adhesions. It shows increase of nuclei of trabeculæ and nerve fibres, and considerable collection of lymphoid cells in trabeculæ. This cell infiltration extends throughout the whole length of nerve examined (a considerable length), but the cells are most aggregated below the lamina cribrosa. There is very considerable infiltration of the outer part of the internal sheath of optic nerve with round cells (lymphoid cells or leucocytes), and the fenestrated membrane has great increase of nuclei owing to this germination. Nerve fibres of optic disc swollen, and lower layers infiltrated with cells continuous with those in trabeculæ of optic nerve. Slight infiltration of ganglionic layer with lymphoid cells. Both granule layers disturbed in regularity and arrangement. Retinal blood-vessels distended with blood corpuscles. Slight infiltration of choroid at optic entrance, at the part immediately contiguous to the cell infiltration of sheath. Arteria centralis has its adventitia very slightly thickened.

Pia mater slightly infiltrated, showing some chronic change.

Brain cortex infiltrated with lymphoid cells (leucocytes), especially in second layer (small pyramidal cell layers). The lymphoid cells can be seen following the tracks of the vessels, and have obviously exuded from them: they are especially in connection with venules, slightly with capillaries. Pyramidal cells so altered as to retain no longer the shape from which they derive their name: they are represented by a collection of three, four, or more small nuclei, and some ill-defined protoplasm. In the first and second layers of cortex are some ill-defined spherical masses, staining purple with hæmatoxylin: they are apparently masses of exudation (plasma or lymph). Nuclei of blood-vessels in places germinating. Several parts of the cerebral cortex were examined and the same changes

Stephen Mackenzie delt. Hartnack. Obj 5, Oc. III.

Section through deeper part of Frontal Convolution showing longitudinal sections of small vessels and branches, filled with coloured and colourless corpuscles, with emigrant colourless corpuscles (leucocytes) collected around the vessels. The exuded corpuscles are represented dark as stained by logwood.

found at each. Similar changes in corpus striatum. Throughout brain-blood-vessels much gorged.

Remarks.—The existence of double optic neuritis without coarse local cerebral disease is a point not only of interest but of considerable clinical importance. Optic neuritis is so commonly symptomatic of some adventitious intracranial product, or of some gross intracranial lesion, that a diagnosis often turns on this one point. That it is not always indicative of coarse intracranial disease cases reported by other observers show. Dr. Hughlings-Jackson, in particular, has drawn attention to this point. In reporting "A Case of Double Optic Neuritis without Cerebral Tumour,"[1] he quotes from a Lecture on Optic Neuritis, delivered in 1871.[2]

"I have been wrong several times in the diagnosis of an adventitious product within the skull, in cases where there had been found double optic neuritis; *but I have far oftener been wrong by neglecting the inferences above stated to be deducible from the presence, or absence, of optic neuritis*—wrong in saying there *was* an adventitious product when the discs were normal, and wrong in saying there *was not* when there was double optic neuritis. I feel, therefore, justified in saying that double optic neuritis does point *very strongly indeed* to coarse disease inside the head [italics in original].

In the case I have narrated I had the benefit of Dr. Hughlings-Jackson's opinion. He kindly took charge of the patient during a short absence, and wrote to tell me on my return that he did not expect "local coarse disease" would be found in the brain after death.

But why should this patient get optic neuritis, the commonly ascribed causes being absent? This raises the question, Why does optic neuritis occur in connection with its usually acknowledged causes—tumours, abscess, &c.?

I think that in spite of the eminence and ability of its advocates, from V. Graefe downwards, the pressure theory of swelling of the optic nerves, whether called choked disc or neuritis, cannot be maintained. We see large encephalic tumours, sometimes of rapid growth, without any neuritis, and we see small tumours in distant parts of the cranial cavity—as in the cerebellum—with power to provoke the neuritis. Neither is there any ante- or post-mortem evidence, other than that of the optic nerves themselves, to bear out the theory of a Stauung's papilla. The inflammation of the optic nerves, which we see with the mirror during life and with the microscope after death, appears to me to be excited in all cases by either an extension process from the membranes of the brain

[1] The 'Royal Ophth. Hosp. Rep.' Vol. VIII., part iii. p. 445.
[2] 'Med. Times and Gaz.,' Sept. 16, 1871, p. 341.

to the optic sheath, and thence to the nerve and retina, from the brain to the optic tract or nerves directly, or by an independent affection of brain and optic nerve by a common cause acting by selection upon histologically allied structures. Of course the inflammatory changes in the optic nerves and discs are modified by the anatomical peculiarities of the part. Vascular dilatation, and serous and corpuscular exudation, though constituent factors in the process of inflammation, do not necessarily advance *pari passu*. In some cases the dilatation of vessels predominates (influenced no doubt by the condition of the vascular system of the patient at the time of the disease inducing the neuritis), whilst in others exudative changes are in excess of vascular dilatation. Hence arise differences in the appearances of the optic nerves, to which many observers attach importance. It is acknowledged, however, by most that these differences have no practical significance as indicating the nature, the position, or the duration of the intracranial lesion on which they depend, and we may therefore conveniently speak of the swelling and exudative changes of the optic nerves under the title of neuritis. To repeat, the neuritis associated with intra-cranial disease is, in my opinion, an inflammation communicated from the membranes, or from the brain itself to the optic nerves, except in the rare cases where they are independent, but due to a common cause. In some cases the implication of the membranes, especially basal, affords a ready explanation of the occurrence of the neuritis. Where the meningitis (tubercular, zymotic, traumatic, or an extension process from abscess, &c.) appears the sole encephalic lesion, this explanation may suffice without bringing in another; but at the same time it must be remembered the *symptoms* of meningitis are due to the changes wrought in the brain, and which are never absent; and so it must be borne in mind that a possibility exists that the neuritis accompanying meningitis may really be an extension process from the implicated brain. Excluding cases of primary meningitis, a secondary meningeal inflammation cannot be brought in to explain the occurrence of neuritis from tumours, as the evidence of such meningitis is frequently wholly wanting. We are therefore thrown back upon some change communicated from the brain tissue to the optic nerves, to explain the neuritis due to coarse cerebral disease, and my impression is that the inflammatory changes found in the optic nerves are in all cases (excepting cases of meningitis) either an extension process from the brain, that is to say, "a descending neuritis," or a common process shared in by the brain and optic nerves. I say my *impression*, because I am not prepared to speak dogmatically on the point without further evidence. I am engaged just now in investi-

gating this subject from the histological stand-point. I may, meanwhile, draw attention to some facts which appear to favour this view. In the first place a tumour in the brain as such cannot give rise to symptoms, except by exciting changes in the nervous tissues surrounding it. Where we have permanent paralysis, there must be destruction of nerve tissue; where there is spasm, pain, vomiting, &c., there must be disturbed nutritive activity associated with tissue changes. Whether the tissue changes around tumours are or are not to be called inflammatory is a question of terminology. There are found vascular dilatation, corpuscular infiltration, thickening of neuroglia, &c., the changes which we should regard as interstitial inflammation in any other organ than the brain. So I take it the changes found around tumours, &c., in the brain are of an inflammatory character, tending to spread in all directions or in certain directions, and in so doing frequently implicate the optic nerves, which really are modified portions of the brain outside the general cranial cavity. Of course I must not be understood to deny that a large and rapidly growing tumour will produce pressure symptoms. My object is to show that pressure cannot be the usual explanation of neuritis from local coarse intra-cranial disease.

The direction the cerebritis takes and the rate of its progression are matters of great moment which require further investigation. Whilst it would at first sight seem probable that inflammatory changes would spread in all directions around the nucleus formed by a tumour, yet it is more probable that the cerebritis spreads most in certain directions—directions of least resistance, of more direct anatomical continuity, &c. We see this in rapid changes as hæmorrhage, and in slow changes as sclerosis. A strong point in favour of the cerebritis which I assume excites optic neuritis, having a tendency to travel in special directions, is the fact that in two cases of uniocular neuritis from cerebral tumour recorded by Dr. Hughlings-Jackson,[1] the neuritis was on the side opposite to the tumour. On any pressure or vaso-motor theory, the neuritis should have been on the same side as the tumour, and the same may be said of the neuritis being set up by intermediation of meningitis. But the inflammation travelling down the optic tracts would explain the occurrence. Such cases of uniocular neuritis are rare, and it would be very valuable if those who have had the opportunity of seeing any completed by a post-mortem examination would place them on record.

If the cerebritis spread in various directions, we should expect the implication of other cranial nerves. It is the expe-

[1] 'A Physician's Notes on Ophthalmology,' Dr. J. Hughlings-Jackson, 'Reprints from Ophth. Hosp. Reports,' &c.

rience of most observers that, whereas amaurosis is common from coarse cerebral disease, deafness is rare, whilst defects of smell and taste, being much less important, are passed over with little notice. As regards nervous deafness, Dr. Hughlings-Jackson has especially remarked upon its scarcely ever occurring in connection with coarse cerebral disease. But it must be remembered that he has (as he says, he fears, in season and out of season) repeatedly called attention to the existence of double optic neuritis without defect of sight; and so it may be that, less open to our examination, inflammatory changes occur in the auditory and other cranial nerves without being detected. It is significant that of the only two cases Dr. Hughlings-Jackson has recorded (so far as I know), in one case he expressly states that he could not account for there being deafness of both sides, seeing there was only disease (tumour) on one side; in the other case he only states (in reprint) that the patient was blind and deaf, and that at the autopsy a tumour was found in one sphenoidal fossa, reaching as far back as the seventh pair of nerves, producing changes in the hemisphere which had reached the auditory nerve. The other cranial nerves, as the third, fourth, sixth, &c., are sometimes paralysed without any explanation of the causation, such as meningitis, &c. It may be in such cases the inflammatory process has reached them from the brain, setting up neuritis.

We know little concerning the circumstances which influence *the rate* at which the cerebritis spreads from the vicinity of tumours. The difficulties underlying this point are very great. We only know of the existence of intracranial tumours at all from the changes excited in the brain-substance around them. We know that optic neuritis may be one of the earliest signs of a tumour, and at the same time we know a tumour may exist for a long time without exciting neuritis. I have watched a patient of Dr. Hughlings-Jackson's, in whom a cerebral tumour was subsequently found, who had symptoms (unilateral convulsions beginning in one great toe) indicating its existence and precise locality several years before neuritis occurred. Nearness to the optic tracts is probably one circumstance which influences the period of occurrence of optic neuritis. Cases of cerebral tumours without neuritis are probably cases in which the cerebritis has not reached the optic nerves.

The present state of our knowledge, then, I hold is not inconsistent with the neuritis occurring in connection with coarse cerebral disease being due to an extension of the inflammation to the optic nerves from the inflamed brain around the encephalic lesion causing the neuritis by direct continuity of tissue, though I am aware how deficient is the

direct evidence of such causation. What is wanted is careful microscopical examination of continuity of cerebral tissue from the tumour to the optic discs in many cases with and without neuritis. I am collecting material for such examination. I would, however, refer the reader to Dr. Clifford Allbutt's work, 'The Ophthalmoscope in Diseases of the Nervous System,' where the subject is discussed with such completeness and ability. Dr. Allbutt, however, draws a wide distinction, both as regards the appearances and causation, between the choked disc and descending neuritis, and believes the latter only to occur by propagation from the membranes or cerebral tissue.

But how are we to explain the cases where optic neuritis occurs without its usual exciting causes? I think the microscopical examination of the brain in my case affords some solution. It cannot have failed to have attracted attention of observers that, apart from neuritis, the changes which occur in the optic nerve and retina are alike in kind with those co-existing in the brain. We see this in Bright's disease, in diabetes, in leuchæmia, in idiopathic anæmia, in ague, in purpura, &c.

In the above case the changes I have described and depicted in the brain are of a kind which in any other organ would be regarded as inflammatory. Fashion is largely responsible for our terminology. Similar exudative changes in the liver or kidney, in the skin, the stomach, or the optic nerves, would undoubtedly receive the name of inflammatory, and we should speak of interstitial hepatitis or nephritis, or of dermatitis, interstitial gastritis, optic neuritis. So, I take it, the changes found in the brain in the above case show a condition of cerebritis or encephalitis. Though to the naked eye there appeared only atrophy of the brain, the microscope showed a corpuscular infiltration of an inflammatory character. But it may be objected, How could the change be inflammatory when the brain was atrophied, seeing that inflammation causes swelling (tumour)? I need only instance interstitial nephritis and hepatitis as examples in which inflammation and shrinking go hand in hand together. The changes need not be exactly contemporaneous throughout the brain; whilst exudation is taking place at some parts, other parts previously affected may be undergoing contraction. The microscopical changes in the above case in the brain and optic nerve are precisely of the same character. We saw with the ophthalmoscope in the eye during life what we could only see in the brain with the microscope after death. As to whether the changes in the optic nerves were sequential to, or contemporaneous with, the changes in the brain, there is no precise evidence to show, though the longer duration of

cerebral symptoms and the shrinking of the brain would incline me to believe that the optic neuritis was secondary—an extension-process from the brain. In Dr. Hughlings-Jackson's patient, to which I have referred, there was no wasting of brain or gross-naked eye change. Dr. Sutton examined the brain microscopically, and reported that sections of the convolutions of anterior lobe showed a large number of nuclei, about the size of white blood-cells, some appeared to be dividing. In places they were collected into groups of from ten to twenty, but they were not especially numerous around the capillaries. Dr. Hughlings-Jackson has since had another case of neuritis without coarse cerebral lesion, which he hopes shortly to place on record.

I think the view I have been advancing—the similarity of the changes in the optic nerves and brain—is of much practical importance, for we can see the optic nerves during life and, I submit, infer similar changes to what we find there in the brain. If this correspondence holds good, the ophthalmoscope may help us to correctly interpret many cases. Thus, in some cases of lead-poisoning we find optic neuritis. When such patients die, all the naked eye can discover is either atrophy of brain or no marked alterations. The test-tube tells us that the brain is saturated with lead. But neither the naked eye nor chemistry tell us what changes have gone on in the brain producing the headache, the delirium, the convulsions, and the coma, which have preceded death. Moreover, we know that under appropriate treatment the optic neuritis and nervous symptoms may coincidently disappear, as in a case which I hope shortly to report in 'BRAIN.' I believe that the neuritis in such cases throws a light upon the character of the cerebral lesion, that the neuritis we see indicates a simultaneous and like affection of the brain, the neuritis being an extension process from the cerebritis, or the two like nervous structures being similarly affected by a common cause—the lead circulating in the blood. I believe that cerebritis will be revealed in such cases by microscopical examination. If these speculations are correct, it shows the extreme importance of the routine use of the ophthalmoscope, apart from any defect of sight, or even sign of nervous disorder. The eye has been happily called the window of the brain. By looking through it with the ophthalmoscope we may often see exactly what is taking place within the cranium, when without this window we should remain in ignorance.

Correspondence.

NOTE ON DR. J. HUGHLINGS-JACKSON'S CASE OF AUDITORY VERTIGO IN APRIL No. OF 'BRAIN.'

THE following sentences occur:—"Things (i.e. things in the room where he might be) began to pass to the right—he said 'from his left eye over his nose.' The same thing kept passing continually in this direction; *reappearing* on the left, and again and again passing to the right; he did not see them *coming back*."

"Each eye was partially and very slightly rotated to the right, in frequent jerks from left to right. . . . I was much interested in hearing him *volunteer* the remark about the mantelpiece, that 'it is going in this way' (he jerking his hand from left to right to show what he meant)."

"Several of my medical friends tell me, referring to my account of the paroxysm I witnessed, that the mantelpiece 'ought to have appeared to move to the left—in an opposite direction to that of the eyes.' That the eyes moved to the right, I am sure; and the patient always told me that things passed to the right."

In thinking over this moving image, it appeared to me that it was produced, not during the jerk or spasm, but during the time of recovery of the eyeball to a state of rest. After each jerk, the eye must move back to its original position before another jerk, and the image then produced would be apparently moving from left to right; that is, in an opposite direction to that of the eyes. The rapidity of the jerk would probably be so great as to prevent the formation of a definite image, hence he would not see the things "coming back."

Dr. Jackson does not say in his paper whether he considers the moving image produced during the jerk, or between the jerks; and as he says he does not see anything discrepant betwixt his report and the direction of the eyes, I suppose he believes that the image is

produced between the jerks; whereas his medical friends think that the moving image was produced during the jerks.

JAMES TAYLOR.

18, Newgate Street, Chester, April 1879.

[My belief is that the apparent movements occurred during the jerks. Dr. Taylor's contrary suggestion is a very ingenious one, and no demonstration can be given against it by the facts of the case I have narrated, so far as they are stated. I believe it is now accepted doctrine that size, distance, and position is an affair of movement, and thus one would expect that displacement of objects in a case of movements of the eyes to one side would be in the direction of those movements. In cases of paresis of one external rectus objects are displaced in the direction of the movement only partly made, but strongly attempted.

J. HUGHLINGS-JACKSON.]

Abstracts of British and Foreign Journals.

ON METALLO- AND MAGNETO-THERAPEUTICS.

THE following pages are a continuation of my previous report on the same subject ('BRAIN,' January 1879), and are intended simply to keep the reader *au courant* of what is being done in the novel field of metallo- and magneto-therapeutics. The subject has entered now upon a new phase, that of physiological investigation; and, so far, the general remarks I ventured to make six months ago have been rather confirmed than otherwise by the results obtained. Heavy blows have been struck at many arguments of the "expectant attention" theory, and the burden of disproof laid at its door. It must be confessed, however, that the *rationale* of the phenomena observed is still veiled in obscurity.

Dr. Beard, of New York, has published some papers (collected as a pamphlet, "Experiments with living Human beings") in which he propounds the principles which ought to govern our reasoning from facts in which involuntary or subconscious life comes into play. The sources of error in such cases may arise from the various interactions of mind and body that are below the plane of volition and consciousness, either in the experimenter or his subject; from conscious or unconscious deception on the part of the subject; from collusion, intentional or not, of third parties; from chance and coincidence. The conditions of experimentation should be:—No repetition of the same experiment upon the same patient; no previous knowledge of the experiment by the subject; no feeling of any kind on the part of the subject towards the experimenter; no display likely to awaken expectant attention; control experiments of every kind.

Dr. Beard thinks that Charcot failed entirely to observe these restrictions, and that his results are phenomena of "trance" in no way ascribable to any electrical action or even cutaneous irritation.

Dr. Sigerson gives us ('British Medical Journal,' Feb. 1 and 8) an elaborate review of the whole question raised by the "recently reported phenomena in connection with Hystero-Epilepsy and Cerebral Anæsthesia." His object is to show that Prof. Charcot and the other eminent men who took part in the Salpétrière experiments did not fall into the gross errors of observations ascribed to them by the "Expectant Attention" party.

1. Much stress has been laid upon the fact that hysteria abounds with sources of deception conscious or unconscious. To this it may be replied that experiments are most successful with patients when the hysterical state is least pronounced, and fail during the paroxysmal periods. It must also be noted that they fail in states of induced lethargy or subjection. On the other hand, marked results have been obtained in cases of undoubted organic lesion without a trace of hysterical symptoms.

2. Hysterical attacks may be produced through physical as well as emotional causes, but in neither case has expectant attention any necessary share in the result. For instance, it has been clearly shown that pressure, quite unexpected, has caused (and removed) paroxysms; and that the same pressure at other times entirely fails, however much the patient's attention may have been engaged in the experiment.

3. In organic as well as hysterical cases there occur crisogenic regions, within the limits of which only can pressure call forth paroxysms. Clinical evidence, as well as observations on epileptic guinea-pigs, show expectant attention to have no share in the phenomena.

4. Dr. Sigerson points out that the Commission of inquiry took more trouble to guard themselves against error than they are generally credited with, and draws attention to the fact that if hysterical patients may occasionally be inclined to promote the success of the experiments, there is no lack of cases where they try to mystify the observer, and show themselves thoroughly averse to the repetition of the process.

A fact that makes the explanation of metallic action through the effects of imagination difficult is, first, that it was never known to the experimenters which metal was likely to act in any particular case; second, that in whatever order they were applied, the same always proved active; third, that in some cases, though the patient was under the impression that the active metal was being applied, no result followed if an inert one was deceptively substituted.

Dr. Sigerson remarks, as I pointed out myself ('BRAIN,' vol. i. p. 560) that it is hardly rational to ascribe to "expectant" attention phenomena not only unexpected, but of the possibility of which nobody would form any idea. He alludes to the transference of anæsthesia. The fact is that metals were avowedly applied with a view to relieve or cure the loss of sensation, and the occurrence of the phenomenon of transfer took everybody by surprise, both physicians and patients.

With respect to the *rationale* of metallic applications, Dr. Sigerson reminds us of the galvanometric observations of Regnard (see 'BRAIN,' Vol. I. p. 546) and the action of feeble galvanic currents.

With reference to magnets, it must not be forgotten that they need not touch the skin, and the patient's eyes being bandaged, they could have no knowledge of the presence or absence of the influencing agent. Yet systematic testing of sensibility showed that it never reappeared except in the neighbourhood of the poles.

5. Dr. Sigerson relates an experiment by Prof. Schiff which exactly fulfils the conditions laid down by Dr. Carpenter. A solenoid is placed upon the anæsthetic finger of a patient whose eyes are bandaged. An observer periodically tests the sensibility, whilst another makes and breaks the current behind a screen, unknown to all concerned. The periods of returning sensibility uniformly coincide with those during which the current circulated. Another experiment of Dr. Schiff's was as follows: A patient was made to breathe through a solenoid, the current being made and broken secretly. After a long period, the right anæsthesia was discovered to be disappearing: the current had just been made. On proceeding to test the left side for the expected transferred insensibility, it was discovered to everybody's surprise that no such transfer had taken place. This result was doubtlessly due to the mesial position of the solenoid.

Prof. Schiff stated that he had observed a case of intermittent coccygeal neuralgia where the contact of, or breathing through, a solenoid invariably brought on a fit of pain. At the same time he refused to attribute the whole of the effects produced to a physical action until similar phenomena had been observed in experiments on animals.

Prof. Schiff has published in the 'Archives des Sciences Physiques et Naturelles' (Geneva, 1879) a contribution to the study of the effect of solenoids on the nervous system. From his own and other experimenters' results he thinks that (in the lower animals

at least) the cortical motor centres are not directly motor but rather sensory, so that the disorders of motion which follow their destruction is due to the loss of sense of tact and position in the limbs so affected. Having then destroyed such centres in several dogs, and allowed some months to pass until the immediate effects of the operation had entirely passed away, he submitted the affected limbs, duly tested as to the anæsthesia present, to the action of a magnetising bobbin. In all the experiments it was found that after a longer or shorter time gentle irritation of the affected paw, such as stroking the hair, &c., was responded to by movements. This result, ascribed to a return of sensation, would sometimes persist for some hours, and could be obtained again daily by repeating the magnetisation. The reader will find a full abstract of Prof. Schiff's important paper in the 'British Medical Journal,' for April 26th, 1879.

Prof. Vierordt (*Centralblatt*, No. 4, 1879) gives the results of a series of experiments made with the view of testing the influence of metallic applications on frogs. The conditions under which he worked were as follows. having removed the cerebral hemispheres, he suspended the animal and stroked or pinched gently the fourth toe with an ivory forceps. In each case six series of observations were taken: in the 1st, 3rd, and 5th the normal irritability was tested; in the 2nd, 4th, and 6th, a plate of zinc (or lead) was applied to the frog. From 30 to 70 excitations were made in each case, and the results tabulated according to the result obtained: (*a*) movement in both legs; (*b*) movement in one leg; (*c*) no movement. Thus it was easy to compare the data, and the general conclusion was that with the application of the metal the effect of irritation was nearly twice as great as without.

The January number of the 'Journal of Anatomy and Physiology' contains a paper of Professor McKendrick's, entitled 'Observations on the Influence of an Electro-Magnet on some Phenomena of Nerve.' The method and results, which are highly suggestive and encouraging to further researches, were as follows:

The sciatic nerve of an ordinary frog-preparation was stretched across the poles of an electro-magnet, and the current alternately made and broken. It was found that there was sometimes a contraction on closure, and usually on opening. These contractions were probably due to an induced current generated in the nerve, and not to any derived current from the battery used, as shown by further experiments with a sensitive galvanometer. It was found, accidentally, that on touching the nerve with a copper wire held in

the hand, contractions ensued when this was done, whilst the circuit was broken, but did not whilst closed.[1]

The theory first assumed to explain this phenomenon was that it was caused by a derived current, due to imperfect insulation from the floor, and apparently was confirmed by the fact that when the operator stood on a glass-legged stool no contraction took place. But further experiments showed this to be erroneous, as the phenomenon did not occur unless, *cæteris paribus*, the nerve was not in direct (or indirect metallic) contact with the poles of the magnet.

Other experiments were instituted with the nerve placed on a glass plate, at right angles to the lines of magnetic force. Enough was observed to show that the nerve was affected, as occasionally the muscles contracted; but this point requires further elucidation.

Professor McKendrick promises us fresh communications upon this highly interesting subject, but for the present does not attempt any explanation.

In the January number of the 'Journal of Mental Science' Dr. Hack Tuke has published an account of his visits to the Salpétrière. The phenomena he witnessed are similar to those described in my first report, but the remarks of the distinguished author of 'The Influence of the Mind on the Body' are of especial value. He points out that the argument against the reality of metallic influence from the fact that in some instances inert bodies have produced similar effects would be very much like denying the action of opium or jalap, because bread-pills are known to have produced purgation or sleep. Extracts from a letter of Dr. Vigouroux are given, in which attention is drawn to the systematically overlooked fact that, besides the experiments upon certain Salpétrière patients who are used for public demonstrations outside the Salpétrière and in non-hysterical cases, experiments have been daily repeated for more than two years on an enormous number of patients of both sexes, in ordinary practice and in the

[1] This fact reminds one somewhat of the unipolar excitation produced by the contact of one electrode connected, for instance, with an induction coil. The latter phenomenon, by the way, seems greatly to have exercised Dr. Sigerson, who, in the paper above referred to, described it as "a curious discovery" made by him two years ago. The human body (even when insulated) in presence of an induction coil may be looked upon as an infinitely great reservoir at zero potential. Each pole being alternately charged positively and negatively, it is evident that there will be currents passing to and fro between the body, and the pole of the coil with which it is in contact. The electro-magnet used by Dr. McKendrick was very large, and perhaps this is why no effect was produced by the insulated operator. The necessity of having the frog in metallic connection with the magnet tends to confirm this explanation.

most opposite conditions. Persons absolutely ignorant of the new methods, experienced the same sensations as those of the Salpétrière, and employed the same terms to characterise them.

In a reprint of his paper Dr. Tuke adds some valuable facts supplied by Dr. Müller, of Graz, who repeated the Salpétrière experiments under circumstances which rendered deception very improbable. Disks of wood, bone, cork, glass, marble were used in one case, among others, for test-experiments; but results never followed unless the active metal, tin, was applied. The usual phenomena of recovery and transfer in cases of anæsthesia, achromatopsia and contracture were observed, and a new fact noted, viz. transference of hemiparaplegia. The Austrian Medical Society investigated those facts and recognised their genuineness. They report further successes in cases of lesions both of motility and sensation.

In presence of such an array of independent witnesses it would be premature, Dr. Tuke concludes, without further experiments, to assume that the influence of metallic applications is solely ascribable to expectant attention.

A case of hysterical hemianæsthesia is described in the 'British Medical Journal' (April 26, 1879), in which the effects of metals and galvanism were tried. The loss of sensibility extended over the whole of the left side, including the special senses. On Nov. 15th, the patient and all around her being quite ignorant of the nature and object of this clinical method, her eyes were bandaged, and plates of copper, zinc, iron and lead applied on left arm, without any result. On Nov. 18th a bracelet of gold coins was similarly applied. The anæsthesia was marked. When removed six hours later, pricking and pinching the arm caused pain. The arm further improved by another application the next day.

Nov. 21st.—A current of 40 Leclanchés was applied first to the posterior and then to the anterior surface of the left thigh for five minutes. At first nothing was felt; but as the skin reddened, pricking became painful. No phenomena of transfer were observed.

Nov. 22nd.—Faradism having been applied by mistake, the patient was found next day to be again completely anæsthetic on left side. From that time the galvanic current was systematically applied to the body, limbs and face, and a gradual improvement was observed, the *anesthésie de retour* after each application never being as complete as it was before the electrisation. This result, however, was only temporary, and the patient relapsed into a state of almost complete hemianæsthesia.

Dr. Anderson, of Edinburgh, relates ('British Medical Journal,' Feb. 8, 1879) a curious case of hystero-epilepsy, with aphasia, which he treated with gold applications externally, and chloride of gold and sodium internally. The fits and hemianæsthesia were considerably improved; but speech still remained in abeyance. The continuous current proved curative of the residual symptoms.

The author made some measurements of the patient's electrical resistance, and found it greatly increased—a fact which by the way entirely agrees with my experience in hysteria, and is but too frequently lost sight of. But it is surely going too far to conclude, from this one observation, to a "specific action" of the selected metal.

A case of metallotherapy is given by Dr. Fieuzal in the 'Progrès Médical' for January 1879. The patient, a girl of 17, complained of amblyopia and dyschromatopsia, with slight paresis of the internal recti. The other senses were found to be also affected. These symptoms, as well as right hemianalgesia and anæsthesia, became so marked as to prevent the patient from going about by herself. Iron, the bromides and hydrotherapy were all prescribed in vain. It was incidentally found that a gold disk restored sensation locally, but this indication was not followed up. An electrical treatment was followed, but to no purpose; in fact the anæsthesia became more marked on the left side as well. At the same time well-marked hysterical symptoms about the throat, &c., appeared. At this juncture Professor Charcot was consulted, and by his advice gold was tried *intus et extra*. The only result was that the patient lost her achromatopsia, whilst her acuity of vision rather tended to become worse. On exchanging silver for the gold plaques, however, a gradual improvement of the special senses soon became evident, along with restoration of feeling in the right side.

A curious case of hysterical blindness, under the care of Prof. Dujardin-Beaumetz and Dr. Abadie, appeared in the 'Gazette des Hôpitaux' (Nos. 55, 56, 1879). The patient, aged 16, had never presented the least sign of hysteria, and was perfectly regular. One night she complained of headache, and the next morning she awoke absolutely blind and anæsthetic. Without giving her any hint of what was being done to her, three gold coins were applied to the left temple. Under their influence there was a slight return of sight in the left eye, visual acuity reaching to 1–10th. Next day a magnet was applied to both temples; but the severe headache it produced prevented its being left long in contact; a slight return of vision ensued. Magnetic applications were repeated for two or three days, but they brought on a state of

lethargy such as to compel their cessation. Gold was then resorted to, *intus et extra*; and other metals successively tried, but without any considerable effect: visual acuity of both eyes 2–5, slight diminution of hemianæsthesia. Static electricity was then resorted to, and with marked effect. After the first sitting the patient could read; and soon both special and general sensation were completely restored. There still remained a tendency to sleep, with occasional fits of lethargy.

A case is related in the 'Gazette Obstétricale,' by Dr. Dupuy, in which metals are supposed to have relieved vesical spasm. The patient, a woman of 40, had suffered from these symptoms for years, and had been cured, apparently, by antispasmodics, &c. She, however, had a severe relapse, during which the pain produced by the passage of the catheter was such as to produce convulsions and loss of consciousness. Such was the horror of the patient for the operation, that she went without drink for two or three days in order to avoid the necessity of daily soundings. Nothing seemed even to relieve this agonising symptom; and Dr. Dupuy resolved to give the metals a trial, especially as the patient was suffering from spasms in other parts of the body. Gold was found to increase the convulsions, whilst copper, silver and iron removed them at once. Disks were then applied, and within an hour the patient passed water spontaneously, abundantly and painlessly. Whenever afterwards micturition did not occur normally, the application of the disks would call it forth, though the act was not always unattended with pain.

Dr. Landouzy ('Progrès Médical,' 4, 1879) relates a curious case of lethargy induced by the application of a magnet. The subject of this observation had just recovered from an attack of severe hysteria, and still suffered from meteorism and other slighter symptoms. Care was taken that she should be entirely unprepared, and her attention was sustained away from the manipulations by means of steady conversation. Two minutes after the application of the magnet to the abdomen, a slight tremor was noticed in the region of the right wrist and labial commissure, and she seemed to fall into a deep sleep. Shaking, pricking, &c., failed to rouse her, but on removing the magnet the identical tremors were observed, and the patient awoke, remembering nothing of the whole affair but the sensation of a cold body on the abdomen. Her eyes were then bandaged again and nothing done to her; no effect followed until the magnet was again brought near her arm. The same phenomena followed as before.

When the magnet was applied to the abdomen, her eyes being opened, nothing but the cold sensation was noticed. If now, however, her eyelids were closed the same lethargy followed. Opening the lids sufficed to awake her. The experiments were repeated several times. *No effect ever followed when a piece of non-magnetised iron was substituted.*

The conclusion of the case of hysterical contracture treated with electro-magnets, of which an abstract is given in the fourth number of 'BRAIN' (pp. 555, 556) appears in the 'Progrès Médical,' No. 8, 1879. The treatment was interrupted during August, on account of an attack of pneumonia. In September it was found that the left arm had lost whatever it had gained, and was paralysed. Dr. Vigouroux now tried a systematic course of statical electricity: the patient being charged and sparks taken from her arm daily for ten minutes. By the middle of January the arm had recovered its full vigour. As the treatment went on, the menses gradually reappeared, and the ovarian tenderness diminished steadily.[1]

Pari passu with these changes, it was noticed that it became more and more difficult to produce artificial contractures by means of the electro-magnet, until at last this became entirely impossible; a proof, Professor Charcot thinks, of the final return of the nervous system to a state of equilibrium.

A case of lead poisoning with hemianæsthesia, treated by magnets, is related by Dr. Debove in the 'Progrès Médical' for February 1879. The patient was a painter who had had two attacks of colic previously. He was picked up insensible in the street and brought into the Hôtel-Dieu, suffering from hemiplegia with anæsthesia on the left side. The electro-muscular contractility was diminished, and during the month of August he suffered from a series of attacks of pain, with periods of coma and delirium. He gradually improved, and in January the only symptoms remaining were loss of power and loss of sensation (including the special senses) on the left side. For this he had been faradised much, but in vain. On Jan. 12th a magnet was applied in presence of Profs. Charcot and Trélat. In a quarter of an hour it was found that the skin

[1] The therapeutical value of statical electricity in certain cases of hysteria seems to be greater than its almost universal neglect would justify. Besides Dr. Vigouroux's case and that of Dr. Dujardin-Beaumetz just mentioned, Prof. Erlenmeyer has also published one lately 'Ctrlbl. f. Nrvhlk.' No. 1, 79), in which the superiority of the old plate machine over the modern batteries and coils, for a certain class of cases at least, was shown.

had become sensitive all over (excepting about the nose and the sole of the foot, where the anæsthesia proved particularly tenacious). The mucous membrane of the mouth remained insensitive. The left eye can count the fingers at 40 cm. and recognise all colours; but there is an annular scotoma.

The improvement continued. On the 6th day the patient can see the fingers at 60 cm. and the hearing was normal on the left side.

In the 'British Medical Journal' for Jan. 18, 1879, Dr. Wilks makes some "Remarks on Hemianæsthesia and its Cure." He contends that neither galvanic action nor mental influence can explain the effects of metals in hysteria; in fact that no explanation can be forthcoming until we are acquainted with the nature of nerve-force. In the same paper he gives the termination of the case referred to in 'BRAIN,' Vol. I. pp. 551, 552 where electricity as well as metals failed to produce any effect. The patient was since re-admitted into the hospital, but no treatment adopted: she was systematically passed over at each visit. One day she stated she was better, and from that time rapidly progressed, leaving the hospital perfectly recovered.

Prof. Benedikt (*Wien. Med. Presse*, Jan. 26, and *Berl. Klin. Woch.* 17, 1879) points out the importance of urine-testing in cases of hysterical spinal irritation. After the therapeutical indications derived thereby have been attended to, galvanism, or metallotherapy, in the shape of chains of zinc disks along the spine, are beneficial. He has also found magnets effective when applied along the cervical spine or even the limbs. The latter may induce a cataleptic state (also obtained by placing the hands over the closed eyes) which is followed by an abatement of the symptoms for several days.

In his Graduation thesis on "the Bilateral Functions in their relation to Metallotherapy" (Berlin, 1879) Dr. Adler concludes from his observations on *two* hysterical patients that the metallic applications act merely by their mechanical power of irritating, and this because a strong mustard-poultice was found to restore sensation and produce the phenomenon of transfer. On the other hand experiments on *five* healthy persons showed that metallic applications had no effect on sensation in some cases, either diminished or increased it in others; whilst a mustard-poultice always increased it at the point of application and diminished it at a corresponding spot on the opposite side. Based upon these scanty and conflicting data, the author apparently wishes to refute

the conclusions of the French Commission. Granted the "mechanical power of irritation" of metallic disks, what about the action of solenoids, &c., at a distance?

The author of an article in the April number of the 'Birmingham Medical Review,' after giving an account of the main experiments and opinions on the subject of metallic and magnetic agency, concludes, that expectant attention cannot account for everything. It does not explain, for instance, the phenomenon of transfer; nor the disappearance of achromatopsia with the same order of colours in every case; nor the fixation of the effect by superimposition of metals, &c. The author very properly invites his readers to preserve an attitude of "expectant attention" towards these observations which, whatever may be their practical value, are of high scientific interest in reference to the influence of metals and magnets upon the human organism.

A. DE WATTEVILLE.

On the Cephalic Ganglion and Retina in the Arthropoda. EMIL BERGER. (*Untersuchungen über den Bau des Gehirns und der Retina der Arthropoden*, § 5, 48. V. Tafeln. Wien, 1878.) By the employment of translucent sections the author of the above paper has investigated the structure of the cephalic ganglion-like masses—which he regards as the brain, in a series of members of the Arthropoda, making the demonstration of their analogy [homology?] to the central nerve system of the vertebrata his main object. It results too, from Berger's observations, that many relations, such as we may recognise among the invertebrata, are capable of opening out to us new points of view as to the structure and functional significance of certain cerebral regions in the higher classes of animals, and above all others this seems to hold good of the central organ of vision. It would be impossible to analyse with due brevity and clearness the details of this most exhaustive monograph.

On the Structure of a Microcephalic Brain. By T. V. ROHON (Vienna, 1879).—The author of this memoir gives the details of his investigations into the structure of the brain of a microcephalic male child, aged fourteen days, which had been kept for several years in alcohol. The results are of especial value, inasmuch as the whole brain was cut successfully into sections fit for microscopical examination. It is impossible here to enter into

elaborate detail, but a few of the principal points only may be noticed.

The cerebrum consists of a single mass without division into hemispheres. On the surface there is only one transverse sulcus; and before it, the rudiments of three longitudinal sulci. There is no trace of a ventricle in the interior. The structure of the cortex does not differ appreciably from the normal.

The corpora quadrigemina and optic thalami lie freely exposed to view. The cerebellum is much better developed than the cerebrum, particularly the vermiform process.

In the interior of the cerebellar hemisphere the nucleus dentatus appears of altogether remarkable size, while the cortex can scarcely be considered as well developed throughout. Corresponding with the defective development of the cerebrum, the crus cerebri is also deficient. The nearer the sections approach the spinal cord, the more normal do the appearances become.

In this case there is an arrest of development which has affected the cerebrum at a very early period, almost at its commencement; while the other regions have become arrested between the third and sixth months of their development.

H. Obersteiner.

On the Termination of Nerves in Striated Muscles.—Tschirjew (*Archives de Physiologie*, 2e série VI.) has employed a new method, suggested by Ranvier, for the investigation both of the centripetal and centrifugal nerves of striated muscles, of which the following are the chief results.

Ranvier's method consists in soaking the tissue in lemon-juice before submitting it to the action of gold chloride, a method which is said to leave the elements of the tissues unaltered. The tissue is placed in gold chloride solution for 10–12 minutes, and then kept in a solution of formic acid of 5 per cent. for ten or fifteen hours in the dark. Or the preparation is exposed to light for twenty-four hours in distilled water acidulated with acetic acid, and then decolorised by formic acid.

The mode of distribution of the nerves in muscle, so far as relates to their appearance and direction, agrees with that described by Kölliker and Odenius. In addition to the vaso-motor nerves, there is in muscle a considerable number of non-medullated fibres which have nuclei in their course, and which frequently form large-meshed plexuses.

They occasionally are seen to join medullated fibres, but more

frequently they join themselves to nerves composed only of two or three non-medullated fibres with large nuclei, slightly thicker than the former, and having a membranous investment. These nerves associate themselves with the medullated nerves, lose their proper sheath, and become lost to view. Frequently also non-medullated fibres are seen which plunge between the muscular fasciculi and disappear.

Tschirjew has used Ranvier's method to trace these nerves to their termination. He finds that they do not end in the fasciculi of muscle, which have no other nervous supply than those of the truly motor terminations.

But on detaching the aponeurosis there is usually seen a large-meshed network of non-medullated fibres. A more minute examination shows that this is not a true network, but an arborisation produced by dichotomous division of the fibres. The individual branches do not anastomose with each other.

The fibres of this arborisation do not possess myeline. They run separate, or run with others similar in a trunk invested by a sheath.

It is easy to show that these fibres do not reach the aponeurosis until they have traversed the whole thickness of the muscle.

The secondary fibrils become more and more fine, and lose themselves in the tissue as very delicate fibrillæ, which in general terminate abruptly, and sometimes present small enlargements like those in the cornea.

The conclusion is that the non-medullated nerve fibres in muscle end in the aponeurosis and are the only sensory nerves of muscle.

Tschirjew has also reinvestigated by the same method the mode of termination of the motor nerves. After reviewing the various researches in this direction, he describes and figures the results of his own researches on different orders of animals.

His conclusions are:—

(1.) The granular cushion with fundamental nuclei which exists in the motor plaques of certain species of animals does not form an essential part of the motor ending, inasmuch as it is not constant in the animal series.

(2.) The terminal arborisation only of the axis cylinder ought to be regarded as the essential part. It may become reduced, as in the tortoise, to a simple terminal knob, occasionally furnished with a nucleus.

(3.) There is therefore no essential morphological difference between the nerve-terminations of striated and unstriated muscle.

Hence the different mode of contraction of these two kinds of muscle cannot be attributed to the differences of the terminations of their nerves.

The Tone of Striated Muscle.—Tschirjew (*Archiv für Anat. und Physiologie—Phys. Abth.* 1879), in his investigations into the nature of "tendon reflex," had arrived at the conclusion that muscles were connected with the central nervous system both by centrifugal and centripetal nerves, which latter enter the spinal end through the posterior roots.

The tendon reflex he found depended on reflex contractions of the muscle, conditioned by mechanical concussion of the nerves, situated at the junction of the tendon and proper muscular tissue. In his histological researches ('Comptes Rendus,' Oct. 1878 *et supra*) he has found a rich plexus of nerves in the aponeurosis of muscles. These nerves at first accompany the intramuscular nerves, but leave them and end in the aponeurosis of the muscle.

The aponeurotic nerves he regards as the only centripetal muscular nerves, but they have no specific sensory function. The phenomenon of tendon reflex depends on irritation of these nerves. He has further inquired whether a certain degree of tension of the muscle, and thereby mechanical stimulation of the aponeurotic nerve plexuses, may not be the efficient cause of muscle tone. The fact of muscle tone has been denied altogether by some physiologists, but Tschirjew, by a series of carefully conducted experiments on the muscles of the rabbit, has shown that, under certain conditions of tension, the muscle becomes elongated on section of its nerves, and also that the curve of a muscle so separated from the centre exhibits elastic oscillations which are not found in muscles whose nerves are intact.

Against the hypothesis of a continuous muscle tone, upheld by Müller, Remak, Brondgeest, &c., it is urged that when the two points of attachment of a muscle are approximated, the muscle becomes quite flaccid. Hence the tone cannot be regarded as continuous, even when all the nervous connections are intact. The tonic contraction is manifested only when there is a certain degree of tension. The facts of ataxy, and anæsthesia without ataxy, are against the hypothesis of a muscle tone conditioned by reflex stimulation from the general sensory surfaces. We may have complete loss of cutaneous sensibility without muscular flaccidity, and vice versâ.

To the reflex tone conditioned by the irritation of the aponeu-

rotic nerves Tschirjew attributes the absence of elastic oscillations which otherwise would accompany every muscular contraction. On this absence of elastic vibration depends the graduation of our muscular movements. He ascribes the inco-ordination of ataxy to the absence of tendon reflex in the muscles and the consequent loss of reflex tone. In ataxy the centrifugal paths are intact as well as the mere elasticity of antagonistic muscles. The cause must be therefore in the centripetal part of the mechanism by which muscular tone is maintained.

When a group of muscles is thrown into action the tone of the antagonists is increased. Hence they offer greater resistance and prevent too great excursion or jerking of the levers moved, and stop the elastic oscillations which would necessarily occur if the resistance was dependent merely on elasticity.

In tabes, in which there is degeneration of the central paths of the centripetal nerves of muscle, or of their tracts of communication with the motor nerves, and hence flaccidity of the muscles, the movements assume a jerking character, and the limb is seen to make a series of wavering or fluctuating movements about the mark, when the patient is told to raise his leg to a given height and keep it there.

D. F.

Note on Infantile Paralysis.—Sections of spinal cord from three cases of Infantile Paralysis were shown at a recent meeting of the Pathological Society of London. These cases not only fill up a lacuna in English work on the subject, but I believe give valuable suggestions as to the progress of this disease, and merit special notice in this Journal.

It has been contended by the French pathologists—Charcot, Vulpian, and Joffroy—that the changes found in the spinal cord, *other* than those in one or both anterior horns, are due to secondary horizontal extension from the lesion in the anterior horn or horns.

The comparison of these cases will, I believe, show that quite another doctrine is tenable.

(1) Dr. Turner's case was as follows: A child, aged 2½ years, six weeks after the onset of infantile paralysis, was taken ill with measles, and died from broncho-pneumonia.

In the grey matter of the spinal cord the maximum of change was certainly found in the anterior horns—more on one side than on the other. But there were extensive changes in the posterior horns, and also in the antero-lateral columns. The changes con-

sisted chiefly in destruction of ganglion cells and infiltration of leucocytes around the vessels, and in groups throughout the grey matter: in fact, there was red softening. Moreover, there was distinct though early sclerosis of the antero-lateral columns, more on one side than the other. There seemed no reason to doubt that many of the changes were contemporaneous, and the important point to note is, that the area of PRIMARY disease extended far beyond the anterior cornua, although there, no doubt, the disease was at its maximum.

(2) In Dr. Taylor's case, twenty months had elapsed after the onset of the paralysis. There was extreme diminution in *all* the measurements of one side of the cord—the diminution being most marked in the anterior horn, where there was almost complete absence of the large motor cells.

In the other half of the cord there were some changes in the central and outer parts of the anterior cornu.

The French interpretation of the conditions found would doubtless have been that the changes, other than those in the anterior horn, were slow, progressive, secondary by horizontal extension from it. But in the light of Dr. Turner's case, it seems fair to ask whether they might not have been the results of an intense acute lesion affecting *primarily* the portions of the cord which were found altered?

In Dr. Humphrey's case, the child died twenty-five months after the period when the paralysis came on.

The changes found were limited to one anterior horn, and consisted almost entirely in disappearance of groups of the large motor cells.

Here it is reasonable to suppose the original lesion was less in extent and intensity. The absence of disease beyond the anterior horn would, at all events, show that there is not *necessarily* horizontal secondary extension, and would be compatible with the view that, if in any given case such extensive disease is found, it may well have been the vestige of the original lesion.

THOMAS BARLOW.

Heredity in Epilepsy.—In the *Archives of Medicine* (vol. i. No. 2; New York: April 1879) Dr. Langdon Carter Gray reports the case of an American lady, aged 40 and married, who was subject to epilepsy, and who transmitted this malady to all her children, nine in number. When twelve years old, she began to have paroxysms in which faces of demons were seen at the right side of

the right eye, causing a great feeling of terror; and up till her twenty-third year she continued to have seizures of this kind, about three times a week. Then epileptic fits of an ordinary character set in, and increased in frequency until they occurred twice or thrice weekly. Since she reached her thirty-fifth year she has had fits only at her menstrual periods. She has borne nine children, four girls and five boys, all of whom have died in convulsions. The effect of the maternal disease upon the children has become more marked as time has gone on. The first three children lived longer (one reaching the age of thirteen months), and had less violent and frequent convulsions. The next four died soon after birth, and had convulsions continuously; the last two lived only for a few hours after delivery, dying in coma.

J. CRICHTON-BROWNE.

On the Influence of Brain Work on the Development of the Cranium and the Brain. (*Gaz. des Hôpitaux*, March 13, p. 243.)—MM. Lacassagne and Cliquet have examined, by the aid of the *Conformateur*, the heads of 190 Doctors of Medicine, 133 rudimentarily educated, 90 illiterate and 91 prisoner soldiers, with the following results:—

		Soldiers.		
Diameters.	Doctors.	Educated.	Uneducated.	Prisoners.
Longitudinal . .	85·29	81·97	79·13	81·10
Frontal . . .	48·91	43·65	42·35	41·12
Parietal . . .	52·58	49·66	50·27	49·90

There is thus a considerable difference in favour of the doctors, and this is especially marked in the frontal measurements. Moreover, the two sides of the head are not symmetrical—in the educated the frontal region is more developed to the left; in the uneducated the occipital region is more developed to the right. The head is larger (more developed), in the case of the educated than in those of inactive intelligence. Among the educated the frontal region is more developed in proportion than the occipital—and, if the difference is greater in the occipital, it is very trifling; while among the illiterate it is considerable.

On the Habitual Tendency of Individuals to Direct Themselves to the Right or Left. (*Ibid.* March 18, p. 253.)—

M. Delaunay has made a communication on this subject to the Société de Biologie, and classes his observations under the following heads.

Race.—M. Delaunay is led to believe that the lower races who generally write from the right to the left incline more to the left than the right, while the higher races direct their course to the right. (This has been observed by *cicerones* in public museums, &c., and is also upheld by the fact that the general rule is to keep to the right on the pavement and similar circumstances, evidently based on the tendency of the majority. One always deviates to the right of a mark when the eyes are shut.)

(M. Delaunay thinks that this tendency to go to the right has played a part in the great migrations of nations. The ancients, who took their bearings looking towards the south, had the west on their right.)

Sex.—Women do not tend to the right so much as men. (A woman takes the left arm of her husband. In meetings where the sexes are separate, the females are on the left, the men on the right, &c.)

Age.—Children commencing to walk incline to the left, till three or four years old, when they take the contrary course. Adults generally go to the right, while, on the contrary, aged people incline to the left. Senile dements at Ville Errard Asylum have been found to promenade the airing courts with the wall on their left always.

Constitution.—Idiots and imbeciles in the same asylum tend to the left rather than to the right. Curable cases and general paralytics take the opposite course. (In Parliaments, literary and scientific reunions, &c., the left represents the party of progress, and the right the retrograde element!)

M. Delaunay believes that the tendency to go to the right observed in individuals advanced in evolution is owing to the left frontal lobe getting the upper hand of the right. He quotes Broca and Roque, showing that the left lobe is heavier, more rich in grey matter and more convoluted than the right; and Lacassagne and Cliquet, showing that the frontal region is more developed to the left than the right in the case of highly educated people. On the contrary, in the case of individuals less advanced in evolution (inferior races, women, children, and aged persons), the right frontal lobe is the chief.

On the other hand, in the case of a well-educated, left-handed, man who always directs himself to the left, the forehead is more

developed to the right than the left. Lastly, the two lobes are equal in those persons who tend indifferently to either side.

Development of the Encephalon after Birth (*Ibid.* April 1, p. 302).—M. Parrot has made a communication to the Society of Biology on the naked-eye appearances of the brain in the first year after birth. He has made 96 autopsies on children, for the most part from the age of two days to four months, and finds the brain a very imperfect and very slowly developed organ.

The anterior part of the brain is developed more slowly than the posterior, and this is in accordance with the early ossifying of the sutures in the latter region. In four-fifths of the cases he found the development of the right hemisphere earlier than the left. The cerebellum is also more rapidly developed than the cerebrum.

On the Hereditary Influence of Alcoholism, and Epilepsy of Alcoholic Origin (*Ibid.* April 26, p. 377).—M. Lancereaux has come to the following conclusions in regard to this important subject:—

Purely functional disorders are frequent if not constant among the descendants of drunkards. This is especially the case with hysteria, and convulsions from such simple causes as intestinal worms.

The effects on the intellectual and moral development are also commented on. It has been pointed out that the children of the workmen of Paris, who consume largely adulterated liquors, have, as a rule, a precocious intelligence, which seldom fulfils its early promise. Among other evils are noted convulsions, epilepsy of puberty, tubercular meningitis and arrested development of the intellect.

Drunkards hand down their depraved appetite to their children, and the tendency makes itself known at the various epochs of life—it may be at puberty, during pregnancy, or at the menopause. Other vicious habits are traceable to the same source.

Besides, there are organic *imperfections* and *changes*, it may be embryonic malformations, inflammatory lesions or atrophies of the nervous centres. But above all epilepsy is the most frequent result of parental alcoholism.

M. Martin has made a study of 150 epileptic patients at the Salpétrière, and of 83 of these the family history has been traced. He has formed two classes.

1. Where the alcoholic tendency of the parents was certified.
2. Where it was doubtful.

The first class includes 60 patients, that is more than two-thirds.

These 60 had 244 brothers and sisters, 48 of whom have had convulsions, while 132 were dead in 1874, and only 112 were alive. Moreover, the greater number of those still living are still young, and several have had serious diseases of the nervous system.

In the second class 23 patients had 83 brothers and sisters; 10 have had convulsions, 37 were dead in 1874, while 46 were yet alive.

In the first group one-fifth of the children had convulsions, more than one-half died early; whilst in the second only one-eighth suffered, and the greater number are still alive.

Finally, 83 families, in which one or more members suffered from diseases of alcoholic origin, had 410 children; and of this number 108 (more than one-fourth) have had convulsions; and in 1874, 169 were dead, 241 lived, but 83 (more than a third of the survivors) were epileptic.

Duality of Cerebral Operations (*Ibid.* May 15, p. 445).—M. Luys has given a lecture on the duality (*dédoublement*) of cerebral operations, and on the *rôle* of the isolated activity of each hemisphere in phenomena of mental pathology. He has considered this subject anatomically, physiologically and pathologically, and the following are his conclusions:—

1. In the normal conditions of the working of the brain the hemispheres are endowed with a certain autonomy.

2. The left hemisphere is earlier in its development and attains a greater weight than its fellow—in general by 5 or 6 grammes.

3. Though the cerebral lobes, under certain conditions act synergetically, there are circumstances under which this does not occur. In spoken and written language the left hemisphere alone enters into action.

4. In playing musical instruments, especially the piano, cultivation creates artificial conditions of cerebral activity by virtue of which each lobe acts separately and independently, producing mental and motor operation—reading the music, marshalling recollections, using judgment, and making co-ordinating movements.

5. In the domain of mental pathology these natural aptitudes for

autonomic activity of each cerebral lobe are susceptible of much exaggeration. In the case of the insane the discrepancy of weight between the hemispheres is much greater than normal, and it is the right lobe which is the centre of troperic activity. Sometimes the difference in weight is as much as 25 to 30 grammes, and that too without destructive lesion.

6. In certain cases of insanity (*hallucinés lucides*) the co-existence of sanity and insanity gives a rational explanation of the integrity of one lobe and the morbid hypertrophy of certain regions of its fellow of the opposite side.

7. In a great number of psychopathic conditions, ungovernable impulses, alienation with consciousness, &c., the morbid states can have no other rational and true physiological explanation than a transient discord between the hemispheres—one acting irregularly, the other normally.

8. With regard to the prognosis of mental disease—the survival and persistence of lucidity implies an integrity of one lobe with all its dynamic capabilities; and reciprocally the absence of this lucidity implies the simultaneous and parallel invasion of both hemispheres.

A. R. URQUHART, M.D.

One-sided Epilepsy with Tubercle in the Motor Zone.—Dr. ASSAGIOLI received a girl of ten years of age into the General Hospital of Venice (see *Giornale Veneto di Scienze Mediche*, Gennajo 1879), who was suffering from pulmonary and intestinal tuberculosis.

During the last days of her life there were convulsive fits confined to the right half of the body. The fits were principally in the leg, but also passed to the arm, neck, and face on the same side. During the attacks, which lasted five minutes, consciousness remained intact, the pupils were dilated. There were four of these convulsions in one day, and one, limited to the leg, two days after. From this till death, eleven days after, there were no more fits.

It was supposed that the lesion was in the cortex cerebri, though the possibility of its being in the ganglia at the base of the brain was not excluded.

On opening the skull there was found hyperæmia of the pia mater, and some irregular pieces of cheesy matter covering a surface of a square centimetre, infiltrating not only the superficial grey substance of the paracentral lobule, but also the underlying white

tissue. The brain substance round about was softened. The rest of the brain was healthy. There were pleuritic adhesions in the chest, crude tubercles in the lung and caseous degeneration of the mesenteric glands. Tubercular ulcers were also found in the intestines.

Una Microcefala: Osservazioni Anatomiche ed Antropologiche del Dottore CARLO GIACOMINI, con quattro tavole litografate. Turin, 1876.—Dr. Carlo Giacomini had an opportunity of examining the body of a microcephale, called Manolino, and comparing the skull and brain with that of two other microcephales, called Bertolotti and Rubiolio. The result of his studies is given in an octavo pamphlet of 90 pages, illustrated by some very fine lithographs of the brain of Manolino and the skulls and cranial outlines of all the three microcephales.

Dr. Giacomini, who has made a careful study of the literature of the subject, enumerates most of the cases of microcephaly which have been published of late years. Manolino died at the age of 17 years. She was 64 inches in height. The head was 4·10 millimètres, about 16 inches in circumference, and the weight of the brain was 550 grammes. This, of course, is not an extreme case of microcephaly, since brains of half that weight have been described. The weight of Bertolotti's brain was 323 grammes.

The intellect of Manolino was very defective, but on this point the details are scanty. The convolutions were broad and simple, and the occipital lobe did not entirely overlap the cerebellum. There was nothing Simian in the type of the brain, and a very careful dissection of the body showed that there were no anomalies which could be traced to reversion of type. The author believes that he was the first to make a careful dissection of the body of a microcephale; this, however, was previously done by Bischoff, in the case of Helena Becker. The results of Giacomini's inquiry confirm those of the German anatomist. Microcephaly cannot be considered an example of atavism, and the only thing Simian about the brain of a microcephale is its diminutive size. At the end of the pamphlet there is a stereographic profile of six skulls, beginning from the outline of a large head, and ending with that of Rubiolio, which is much the smallest of them all.

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